SCIENCE

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THE AMERICAN SOCIETY OF NATURALISTS.

HOMO SCIENTIFICUS AMERICANUS.*

MEMBERS of a society of naturalists, which includes psychology and anthropology in its scope, are familiar with the part played by rites and ceremonies in social evolution. Millions of holy shrines have been red with the blood of victims, and from countless altars the smell of burnt offerings has risen to heaven, in order that men should be united in closer bonds. We have not broken with the past; we meet together at our annual feast; the lot has fallen on your human sacrifice. Your social service is witnessed by the fact that you do not send him a scape-goat into the wilderness, but come to share his suffering.

The objects of our society, as I understand them, are not the increase of facts and the discussion of theories in each of the natural sciences. Our aim is rather to be

* Address of the president of the American Society of Naturalists, read at the annual dinner, Washington, D. C., January 1, 1903.

a center of organization which shall concern itself with the conditions that are essential to the advancement of science. Our annual discussions and addresses have as a rule treated questions of this character. We ourselves are such a problem. Men of science are the central factor on which scientific progress depends. This is indeed so obvious that we are apt to take ourselves for granted, directing our attention to external conditions that are in fact of far less importance. The remote and the abnormal first attract the curiosity. Psychology is the newest of the sciences and astronomy the oldest, though we really know more about ourselves than we ever shall know about the stars. We are indeed so familiar with the rich complexity of our perceptions, thoughts, feelings and actions that it is difficult to make those artificial abstractions which we call a science. The stars are so far away, atoms are so invisible, we know after all so little about them, that astronomy and chemistry may become exact sciences without contradiction. But if there were as many chemical elements as there are people, the discovery of a new element would be no more important for science than the birth of a new baby in the negro quarter of Washington; if the stellar systems were all visible, the discovery of a new satellite would be of no more interest to the world than the mother's sight of her baby's last tooth. The play of a child for a single day is more complex than the known performance of the stellar universe, and each child changes every day and is different from every other child.

But when the psychologist finds that biology has had the patience to define a million species he may take courage. The differences between individuals or between classes of individuals may be as valid for psychology as are species and varieties for zoology and botany. If it is said that the differences in men of the same race are too obscure and shifting to admit of classification, it may be replied that this can not be settled before an inductive study has been made. If it is said that human differences depend chiefly on the environment of the individual, it may be replied that it is a scientific problem to determine what depends on heredity and what on environment, and that the investigation of the effects of environment may be not less interesting scientifically and more important practically than the study of traits that are beyond control. I for my part do not hesitate to claim that the differences between Shakespere and Darwin are as great as those between *Aspasma minima* (Döderlein) and *Aspasma Ciconiæ* (Jordan and Fowler),* and that it is as nearly in our power to develop an *Aspasma Ciconiæ* (Jordan and Fowler) from the egg of an *Aspasma minima* (Döderlein), as to turn the baby next at hand into a Darwin.

Science is inclined to be somewhat conventional in the subjects it considers, holding fast to an orthodoxy of its own. Once it was a burning question as to how many angels could dance on the point of a needle; now we become equally warm on the subject of the number of species in a given genus. There is no obvious reason why we can not consider with equal propriety how many different kinds of scientific men there are. A tentative classification must precede a study of distribution and life-areas, and when this has been accomplished we shall be in a position to take up the natural history or ecology of men of science.

Following the preliminary work of de Candolle and Dr. Galton, I have for some time been engaged in an investigation of

* "This species is distinguished from *Asparma* [sic] *minima* by the ends of the dorsal and anal reaching the caudal and thus their bases are upon the caudal peduncle; it also differs in the larger number of fin rays." Jordan and Fowler, *Proceedings of the National Museum*, XXV., p. 415, 1902.

the scientific men of the United States. I am selecting a thousand of them for subjects, and have at the same time chosen for similar study the thousand most eminent men in history and a thousand students of Columbia College. Each of these three groups seems to me favorable for such work. The students of Columbia College are measured, tested and observed in my laboratory; we are able to follow their academic courses and their careers in after life. The lives of the most eminent men of history are to a certain extent public property, open to statistical investigation and psychological analysis. A thousand scientific men in the United States would doubtless be willing to assist in furnishing the material needed, which is in any case accessible from other sources. It was at one time my intention to base this address on an inductive study of these scientific men. My reasons for not doing so are similar to those of a friend who was asked why he did not bring his wife to this dinner. He replied that he did not suppose that women were welcome, and besides he was not married. I fear that statistics would be rather out of place and unpresentable on such an occasion, and besides I have not the statistics. I am, however, trying to get them, and not being able to find a more satisfactory subject for my remarks, I must ask your permission to say something in regard to ways and means and such preliminary results as are at hand.

I have been aided in collecting data for my work by the preparation of a biographical catalogue of the living scientific men of the United States, for which the Carnegie Institution has defrayed part of the clerical expense. This additional task has, however, delayed the completion of my work, owing to the increased mass of material that has accumulated. There were on my preliminary list more than 8,000 names, and after those who have not done research work in the natural and exact sciences have been eliminated, there still remain some 4,000 scientific men in place of the one thousand with whom I had intended to deal. It was to me surprising, as well as gratifying, to find that our men of science are so numerous. Brown Goode estimated in 1886 that the number of scientific men in the United States numbered about 500, and Dr. Galton estimated in 1874 that those in the British Islands 'would amount to 300, but not to more.' If these estimates were correct, there has been a noteworthy increase in the number of scientific men, and it appears that the Gauss-Quetellet curve of distribution does not hold for scientific eminence, as we certainly have not ten times as many eminent scientific men as there were in Great Britain twenty-nine years ago. Perhaps we are on the average as competent, and only less eminent because there are so many of us among whom this quality must be divided.

As I have already indicated, our first step in the study of scientific men must be to classify them. Logical classifications of the sciences have been attempted, but with only tolerable success, at least beyond a threefold division into the physical, biological and mental sciences. These divisions are fairly valid—the physical sciences being primarily quantitative and independent of the others; the biological sciences being primarily genetic, but dependent on the physical sciences; and the mental sciences being largely analytic and speculative, but, when properly developed, being both quantitative and genetic, and depending on the physical and biological sciences. Even this broad division, however, must break down —the physical sciences must become genetic and the biological sciences must become quantitative; and the divisions are

interdependent, a science, such as physiology, for example, obliterating the distinctions.

On the whole we may expect to secure the best classification as the result of an inductive study of scientific men. Classifications have, as a matter of fact, resulted from natural selection in the development of scientific courses of instruction, books, journals, bibliographies, societies and the like. I have made compilations of this character, which enter too much into detail to present here. I have also let the scientific men of the United States classify themselves. As a result of this inductive study, but doubtless also under the influence of logical considerations, I have made the classification of the sciences shown on the chart which I have distributed.

The graphical representation of the relations of the sciences on a plane turned out to be more feasible than I had anticipated. It must not, however, be understood as intended to assign separate plots to the sciences, but rather to show their complex interdependence and to indicate some of the classifications that can be made. Mathematics, both in its methods and in its relations to the other sciences, occupies a unique position, which can be indicated by placing it above the sciences and nearest to those that are the most exact. Physics, chemistry and psychology are the fundamental sciences on which all the others depend. Physics-chemistry at one side of the chart and psychology at the other indicate this graphically. The facts of matter in motion or energy and the facts of consciousness are more clearly distinct than any other phenomena, while at the same time, being abstractions from the same experience, they are absolutely interdependent. Physical science presupposes perceptions and reasoning, while minds are conditioned by the physical world. Physiology, whether or not life be regarded

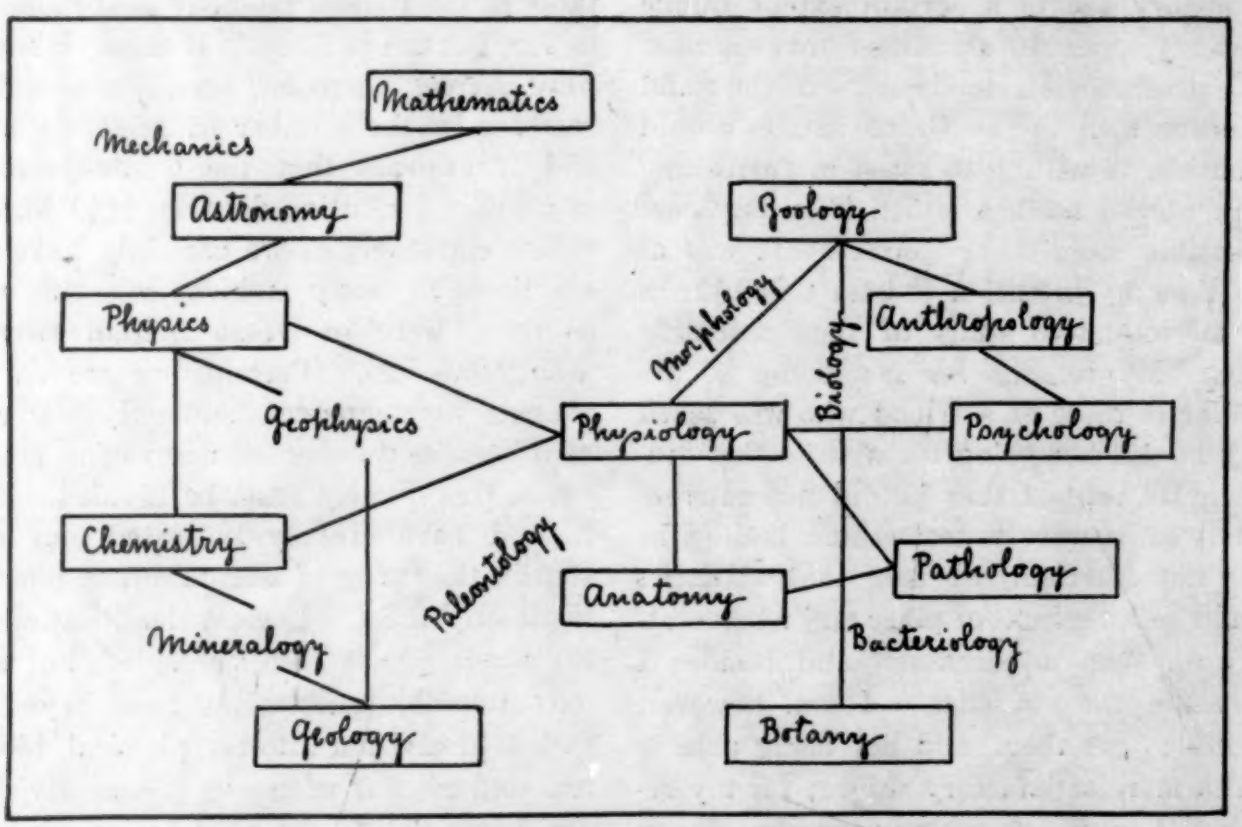

CHART SHOWING THE RELATIONS OF THE SCIENCES.

as a manifestation of a special form of energy, occupies a central position, forming one triangle with physics and chemistry, and another with zoology and botany. It also forms properly a triangle with the secondary sciences, astronomy and geophysics, and parallels psychology. The great natural sciences, geology, zoology and botany, form a triangle, and their interrelations with the other sciences are indicated with tolerable accuracy by their places on the chart. Anthropology, anatomy and pathology might be included logically under zoology or botany, but owing to their actual development they deserve distinct places. Several other sciences in their interrelationships are indicated on the chart. There might also be entered further to the right sciences such as philology, sociology, history, etc., dependent primarily on psychology, but also on the material sciences. On a plane above that of the chart might be put the applied sciences—engineering, agriculture, medicine, education, etc., each of which rests on a large number of pure sciences, but has specially close connections with one or more of them.

Limiting the scope of this inquiry to the twelve sciences indicated, the men of science of the United States are distributed by various agencies, as shown approximately on the table. On the lower half of the table the same figures are reduced to the common standard of a thousand in each class. Chemists, zoologists and physicists are the most numerous, each group containing about one seventh of our scientific men. About one man of science in ten is a geologist, a botanist and a mathematician; about one in twenty a pathologist and an astronomer. In decreasing numbers then come the physiologists, the psychologists, the anatomists and the anthropologists. It should be noted that the Chemical Society is large, and degrees in chemistry are numerous because chemistry is an applied as well as a pure science. Teachers of mathematics and of the medical sciences are numerous because these are required subjects of study; original contributions are scanty as compared with the numbers of those who teach them.

TABLE I.

THE NUMBER OF AMERICAN MEN OF SCIENCE AND THEIR DISTRIBUTION AMONG THE SCIENCES.

	Special Societies.	Fellows of Association.	Members of Academy.	University Professors.	Doctorates in Five Years.	Contributors to SCIENCE 13 Vols.	Who's Who.	Biographical Dictionary (estimated).
Mathematics.	375	81	1	136	61	35	46	380
Physics.....	149	167	23	105	69	155	73	556
Chemistry....	1933	174	12	143	137	73	166	656
Astronomy...	125	40	12	41	16	48	51	212
Geology.....	256	121	13	55	32	161	174	436
Botany.....	169	120	7	57	53	94	70	416
Zoology.....	237	146	17	83	72	243	131	620
Physiology...	96	10	2	53	18	22	25	156
Anatomy.....	136	10	0	56	1	13	18	116
Pathology...	138	14	5	68	4	44	56	224
Anthropology.	60	60	3	4	5	56	37	92
Psychology...	127	40	1	37	63	58	21	136
	3801	983	96	838	531	1002	868	4000

REDUCED TO PER THOUSAND.

Mathematics.	99	32	10	162	113	35	53	95
Physics.....	39	170	240	125	128	155	84	139
Chemistry....	506	177	125	171	265	73	191	164
Astronomy...	33	41	125	49	30	47	59	53
Geology.....	68	123	136	66	60	161	200	109
Botany.....	45	122	73	68	99	94	81	104
Zoology.....	63	149	177	99	134	243	151	155
Physiology...	25	10	21	63	34	22	29	39
Anatomy.....	36	10	0	67	2	13	21	29
Pathology....	36	14	52	81	8	44	64	56
Anthropology.	16	61	31	5	9	56	43	23
Psychology...	34	41	10	44	118	57	24	34

The statistics that I am collecting will give more valid data in regard to the number and distribution of the men of science of the United States than any of the other classes. Next in importance, representing as they do the future rather than the past or present, are the degrees of doctor of philosophy conferred. It has taken during the past five years, in round

numbers, fifty professors of anatomy and sixteen professors of pathology to produce one new investigator; it has taken more than two professors of mathematics or of astronomy to produce an investigator. Each professor of chemistry, zoology and botany has produced one investigator. Psychology has the best record, each professor having produced two investigators. I may be pardoned for referring with gratification to the promise of my own science. The membership of the National Academy of Sciences seems to be the most erratic of the data. Approximately one in twenty of the astronomers and physicists of the country are members of the academy, one in sixty of the botanists, one in a hundred of the psychologists, and one in four hundred of the mathematicians. It is obvious that the Gauss-Quetellet curve entirely fails in its application to the distribution of scientific ability, that eminence may be obtained with much less ability in some sciences than in others, or that some sciences have been favored in elections to the academy.

I am at present engaged, as I have already stated, in a statistical study of these scientific men. I am putting on cards certain data which it will be worth while to summarize. Thus the distribution of men engaged in the several sciences in different parts of the country and its relation to the total population, the relative numbers in large centers, connected with institutions of learning, etc., the comparison of the present location with the place of birth, the education, the ages, the amount of shifting from one institution to another, the rate of promotion, the character and quantity of research, etc., of these scientific men will have a certain interest. This interest will be enhanced and become more truly scientific in character if similar statistics are collected for other countries and for this country at periods of ten years.

This must be left to the future. I am, however, proceeding with work which I trust has a certain scientific and psychological value. I am selecting from all those who have carried on scientific research the thousand whose work is regarded as most valuable. The numbers chosen from each science are in proportion to the total workers in that science. I am asking representatives of each science—selecting those who are most eminent and who are at the same time believed to be familiar with the conditions—to arrange the students in that science in the order of merit. It is obvious that this can only be done approximately. There are diverse lines of research in each science which it is difficult to compare, and there are various ways of contributing to a science which are scarcely commensurable. It strikes some that we are in the condition of the boy at his geometry lesson who when asked what follows when two sides of a triangle are equal replied that all the other sides would be equal too; or of the man who when asked if he did not think the story of the *raconteur* in his anecdote a little broad said he did not think it was as broad as it was long. It is, however, the business of science to overcome insurmountable difficulties; and it is one of the triumphs of science that it can in certain cases measure our ignorance as well as our knowledge.

If the workers in a science are arranged in the order of merit independently by a number of observers, the average position of each can be found and its probable error calculated. Thus we can say that to the best of our knowledge a man stands eighth among our mathematicians and that the chances are even that his real position is between sixth and tenth. The same man might stand eightieth among our scientific men with a probable error of twenty places. The probable errors show that the order of arrangement has validity within

certain limits and tells us what this validity is. The sizes of the probable errors increase rapidly as we go down the list, thus proving some degree of approximation to the theoretical curve, based on the assumption that scientific merit and eminence are dependent on a large number of small causes and giving us the data for the construction of the actual curve.

I am certainly under great obligations to those who assist me in making the arrangements; some find it interesting, others irksome; all show a certain amount of reliance on my discretion. The individual lists will of course be used only for the averages and probable errors, and no record is kept of those who make them. I could doubtless give this address a *succès de scandale* by reading to you the order of merit so far as ascertained, but I have no intention of making public the list until such time has elapsed that each may assume that if the process were repeated he would stand at the head. But while the list may not be published, it is possible to draw from it certain deductions of scientific and practical value. The statistics of the whole number of scientific men have greater interest when compared with those of the more eminent thousand. We can tell whether the average scientific standard in one part of the country, at a given university, etc., is higher or lower than elsewhere; we can give quantitatively, the men being weighted, the scientific strength of a university or department. It would be possible to determine more exactly than by existing methods who should be a fellow of the American Association or a member of the National Academy. It is possible to correlate age, education and other factors with scientific eminence.

The selection of the thousand scientific men who are thought to have done the best work, and their arrangement in the order of merit, are somewhat incidental to my main object, which is to secure a group sufficiently large and homogeneous for scientific study and for comparison with other groups. The problems that are opened and may ultimately be solved are numerous and not unimportant. The old question of the relative contribution of heredity and environment to the making of the individual must for men be solved by a study of men. The infant is more plastic and his surroundings are more varied than is the case with other animals. We may find that the mathematician must be born, whereas the naturalist can be made by a sea voyage. The little scientist can doubtless be made, but probably the great man of science must be born. We have to determine what conditions of both nature and nurture are favorable for the production of usefulness and greatness in scientific work. We should like to know at what age the future of a man can be foretold with a given degree of probability, at what age he has his most original ideas, at what age he does his most efficient work, at what age he is likely to become a public nuisance. We want to know what conditions of health, habits, family, employment, rewards and the like are favorable for scientific performance. In general, we should like to find out how we scientific people differ among ourselves and from others, to make a natural history of scientific men and to use the knowledge for the improvement of the breed.

I have made a small beginning in the direction of getting a scientific description of men of science. At the present convocation of scientific societies I have arranged an anthropometric laboratory in which certain physical, psychophysical and mental tests are being made. These will show how in some fundamental traits scientific men differ among themselves and from other groups.

Certain other traits I am attempting to

estimate and grade. These are shown on the second table. The terms are partly self-explanatory. We know what is meant by physical health and mental sanity and balance. The three next categories follow the analysis of consciousness current in psychology. We distinguish three aspects of mental life—the cognitive, the emotional and the volitional. One of these may be particularly well developed. The man of science must perceive correctly and reason clearly; the artist must have vivid emotions; the statesman or soldier must have a strong will and be prompt to act. These traits are not exclusive of one another, as is usually assumed. The eminent man of science is far more likely than the average man to be a poet or an efficient executive officer. This may be because the traits are correlated, or it may be because the man of achievement must excel in various traits which have been accidentally united in him. This is a question of considerable scientific and practical importance, but one too new and technical for discussion here. I may, however, state that in the Columbia tests we have found a lack of correlation; for example, the man who has a good memory is not more likely than another to be accurate or quick in perception.

The next group of qualities is adopted from my own work in psychology. I have tried to prove by experiment that mental processes vary in time, in intensity and in extensity, that these magnitudes can be measured, and that they are correlated with the time, energy and space relations of the physical world. These fundamental quantitative categories appear to be applicable to character as a whole—a man may be quick or slow, strong or weak, broad or narrow. These qualities seem to me to define and render more exact the four temperaments which are almost the only types of character that have obtained currency. Thus the choleric man is quick and

Table II.

GRADES FOR DIFFERENT TRAITS ASSIGNED TO FIVE MEN OF SCIENCE.

	A.	*P. E.*	B.	*P. E.*	C.	*P. E.*	D.	*P. E.*	E.	*P. E.*	Av.	Av.
Physical health	63	*6*	55	*4*	26	*6*	90	*3*	12	*4*	49.2	4.6
Mental balance	84	*3*	79	*3*	32	*6*	45	*6*	20	*4*	52	4.4
Intellect	90	*3*	57	*4*	79	*3*	38	*6*	49	*3*	62.6	3.8
Emotions	13	*5*	26	*4*	24	*5*	26	*4*	55	*6*	28.8	4.8
Will	90	*4*	45	*3*	49	*4*	63	*3*	2	*3*	49.8	3.4
Quickness	87	*3*	57	*3*	99	*1*	9	*6*	33	*5*	57	3.6
Intensity	82	*2*	25	*4*	76	*3*	57	*5*	8	*3*	49	3.4
Breadth	93	*3*	74	*4*	38	*6*	63	*3*	68	*4*	67.2	4
Energy	98	*1*	32	*4*	90	*3*	77	*3*	3	*4*	60	3
Judgment	96	*2*	70	*4*	30	*3*	30	*5*	15	*6*	48.2	4
Originality	82	*3*	17	*3*	84	*4*	66	*4*	8	*5*	51.4	3.8
Perseverance	96	*2*	30	*4*	54	*5*	87	*3*	1	*1*	53.6	3
Reasonableness	67	*5*	93	*2*	20	*4*	38	*6*	20	*6*	47.6	4.6
Clearness	90	*3*	74	*4*	72	*4*	17	*4*	45	*6*	59.6	4.2
Independence	94	*4*	57	*3*	72	*4*	52	*5*	5	*5*	56	4.2
Cooperativeness	63	*4*	49	*4*	19	*4*	38	*7*	10	*6*	35.8	5
Unselfishness	38	*4*	67	*3*	10	*4*	45	*6*	17	*6*	35.4	4.6
Kindliness	45	*7*	82	*2*	10	*4*	54	*6*	48	*5*	47.8	4 8
Cheerfulness	48	*5*	77	*4*	34	*5*	34	*7*	26	*5*	43.8	5.2
Refinement	52	*4*	72	*4*	8	*3*	4	*3*	63	*4*	39.8	3.6
Integrity	96	*2*	87	*2*	38	*6*	76	*4*	38	*7*	67	4.2
Courage	95	*3*	52	*3*	51	*5*	45	*5*	12	*4*	51	4
Efficiency	100	*1*	57	*3*	74	*3*	34	*4*	4	*4*	53.8	3
Leadership	87	*2*	20	*3*	17	*4*	6	*4*	6	*5*	27.2	3.6
	77	*3.3*	56.4	*3.3*	46	*4.1*	45.5	*4.6*	23.6	*4.6*	49.7	4

strong, the phlegmatic man is slow and strong, the sanguine man is quick and weak, the melancholic or sentimental man is slow and weak. But any one of these types may be broad or narrow, and this seems to be as characteristic a distinction as quickness or intensity. Further, these characteristics may vary in different degrees; the men called phlegmatic are slow, but not to the same extent; they vary more in strength and still more in breadth.

The descriptive terms that follow have been selected from a large collection that I have made and are intended to cover the ground as completely as may be with a limited number. The whole plan is as yet tentative and is doubtless open to improvement toward which I shall welcome any suggestions.

It is my intention to grade and to ask others to grade scientific men for these various qualities. It is not necessary to enter here into details of method. I submit, however, on the table the grades that have been given to five of those entitled to be present at this dinner. The grades were assigned independently by twelve observers acquainted with the men, and have been adjusted and distributed on the supposition that the group of individuals and the distribution of the traits represented average values. The grades are arranged on a scale of one hundred and probable errors are attached. The probable errors, though assigned by the usual formula, are, I think, too small; but they are correct relatively and show which traits are judged with greatest unanimity. We have seen that there are about 4,000 scientific men in the United States. A grade of 100 for efficiency means that the man is thought to stand among the forty most efficient scientific men of the country. A grade of 26 for integrity does not mean that a man is not honest, but that this trait is less marked in him than in three fourths of scientific men.

It may seem unkind, even inhuman, to grade men as though they were prize cattle at a county fair. It is sometimes said that modern science has banished mystery, romance and beauty from the world. But this is not true. The physician smokes too much, the obstetrician falls in love, and even the psychologist makes a fool of himself much as any other man. The rainbow is not less beautiful because Iris has been pierced by the refracted rays from the sun, nor is the universe less grand because Phœbus and his horses have fallen before the law of gravitation and the concept of order. We can not now design cathedrals, but we can build steamships and bridges that are beautiful.

It is our business to make both a science and an art of human nature. As in the physical world we select first the material suited to our purpose, then turn the iron into steel and temper the steel for the knife, so in the world of human action we must learn to select the right man, to educate him and to fit him for his exact task. This indeed we try to do in all our social institutions, religions, commerce, systems of education and government. But we work by the rule of thumb—blind, deaf and wasteful. The nineteenth century witnessed an extraordinary increase in our knowledge of the material world and in our power to make it subservient to our ends; the twentieth century will probably witness a corresponding increase in our knowledge of human nature and in our power to use it for our welfare.

Lest, in spite of sporadic efforts to the contrary, this address makes the impression of a scientific paper rather than of an after dinner speech, I shall conclude with certain speculations which may or may not be upheld by an inductive study. It seems to me that scientific men suffer in character

because they are employees rather than free men. We are not permitted to follow our chosen leaders, but men are placed in authority over us. We are paid to teach or the like; our scientific work must be done almost clandestinely. We do not earn our livings and support our families by the results of our real work; we are grateful if some charity will publish them for us. The pleasure of discovery, fame and honor are supposed to be our reward. Every normal man finds his chief pleasure in doing his work well and with the appreciation of others; but under existing social institutions the value of his work and the approval of his fellows are usually measured by his freedom and his income. The scientific man, seeking the truth without regard to consequence, should be more fearless, simple, fair and kindly than others. In so far as this is not the case, it appears to me to be due to the conditions under which he works.

Evolution has progressed by the survival of the strong and the cunning, of those armed with tooth and claw, of those quick to run and ready to hide. It has given us the vulture and the parasite. Human history has left us the legacy of the iron hand and the crooked back. The man engaged in scientific work has too often filled the position of an upper servant—a tutor to the sons of the rich, a priest subscribing to tenets that are outworn, an employee dependent on the favor of presidents and boards,—for whom silence is silver and flattery gold. As the downtrodden have submitted to servitude on the ground that they will have their reward in a future life, so scientific men have labored in the hope of recognition and posthumous fame. They have scrambled for degrees, titles, membership in academies and the like, trying to climb up on each others' necks. But the things that have been are not the things that shall be. The men who labor with their hands have learned to unite in trades-unions; they have shown themselves ready and able to make the utmost sacrifices for their common cause. And they have won; they have used the governors of states and the president of the United States for their purposes. Their leader can speak to the president on terms of equality; the members of the National Academy of Sciences waited last spring for an hour in the anteroom of the White House until he did them the honor to shake hands with them. Is there a university in the world whose faculty would resign because one member was unjustly treated, or would scientific men subscribe ten per cent. of their incomes to support a faculty that had so resigned? But the things that have been are not the things that shall always be.

Scientific men will not forever submit to being embroidered with gold braid and bound with red tape. The diplomacy and intrigues of courts are slowly giving way to the rough and ready ways of democracy. For a time there may be confusion and some waste; but on the whole there is more promise in the man in shirt sleeves than in the conventional gentleman. I believe that it is our part here in America to form a true democracy of science, where each will do the work for which he is best fit and will receive the reward that he deserves, where we shall choose our own leaders and follow our leaders because we recognize them as such. I am myself an optimist. I am sure that the time will come when scientific work will assume the position that belongs to it. The time will come when there will be peace and good will on earth, and all things will be managed efficiently and in accord with the pure light of reason. I am indeed so much of an optimist that I am glad to live in a period of transition and turmoil, rather than in the millennium for which we strive and suffer.

J. McKeen Cattell.
Columbia University.

*HOW CAN ENDOWMENTS BE USED MOST EFFECTIVELY FOR SCIENTIFIC RESEARCH?**

I AM not sure that I have rightly apprehended the special phase which it is desired the discussion should take, if, indeed, it is desired that it should take any one trend rather than another among those that are perhaps equally embraced under the broad theme announced. I have interpreted the question as though it read—*By what assignment of endowments can research be most effectually aided?*

I assume that, with some rare exceptions, endowments may be welcomed in whatever form they may come, but that their effectiveness may be much greater or much less according as they are judiciously or injudiciously placed. Some endowments, indeed, may be so hampered by restrictions that they are better declined than accepted, but these, it may be hoped, will grow more and more rare as intelligence relative to what is wise in the endowment of research increases.

I assume that the principles which control modern success in most other enterprises will be found applicable in general to the endowment of research, and that among these principles are specialization in subject, careful choice of talent, the largest possible use of the highest talent, the greatest possible avoidance of inferior talent, concentration of effort by institutions of limited means and coordination of effort between institutions of whatever means, rising to close combination in effort whenever practicable.

1. *Endowment of Chairs of Research.*—The time is fully ripe for the special endowment of chairs of research. The promise of results from such endowment is very great. Not a few of the chairs in our leading universities are devoted chiefly to research, but this is usually due more to the personal force and peculiar endowments of the occupant than to self-determined provision on the part of the institution or its patrons. The chairs that are endowed primarily for research are very rare. There ought no longer to be a struggle on the part of the capable investigator to free himself from obligations to teach that he may devote himself to creative work. It need not be urged here that creative work is more serviceable to mankind than expositional work or even disciplinary training. Real capability for investigation of a high order being granted, all pressure from the institutional environment should be such as to impel the investigator to give himself as undividedly as possible to research.

The endowment of *chairs* of research is here first urged, not because it is superior to the modes of endowment yet to be considered, but because it requires but a moderate gift, as gifts now run, and is, therefore, within the reach of a large number of possible patrons, to whom endowments in more than six figures are impossible. From $100,000 to $200,000 will effectively endow a chair of research from which great results in time may be expected. Endowments of less amounts may be made to provide that a specific fraction of the time of the occupant of a chair shall be devoted to investigation, and thus creative work may be effectively promoted by a modest sum.

2. *Endowments for Departmental Research.*—No scientific staff of a university should regard itself as fulfilling its mission in any adequate way if it does not devote an appreciable portion of its energies to investigation. At present the provision for research is usually rather vague and

* The annual discussion before the Washington meeting of the American Society of Naturalists, January 1, 1903. Dr. W. H. Welch, of Johns Hopkins University, also took part in the discussion, speaking with special reference to the Rockefeller Institute for Medical Research.

uncertain, if, indeed, there is specific provision for it at all. While research is theoretically recognized as a proper function and is perhaps cordially appreciated by the authorities of the institution, there is in actual practice a constant struggle between the demands of instruction and the desire for investigation, in which the preponderance of pressure growing out of the rapid growth of most universities too often lies on the instructional side. Relief for research is to be found in endowments specifically devoted to the purpose. It is here proposed that the endowment shall be made to the *department* rather than to a specific *chair*. The application of the revenue is, in this case, broader and more plastic than in the endowment of a specific chair. The function of research may be distributed among the members of the staff according to their capabilities and tastes, and thus give to them something of the touch and inspiration of creative work, while they may still retain an instructional function of greater or less degree; or it may be concentrated at one time in a given line by a given member, and at another time in a different line by another member, as conditions may favor. It is not in all cases—perhaps not in most cases—altogether best for the investigator to be relieved entirely from instructional function, since the exposition of his work has its good effect in forcing the organization of his thought. The critical review of it, as he assumes the obligation of presenting it to younger minds, is wholesome, as are also the questions and discussions incidental to such presentation. But the amount of such profitable instructional work has rather severe limitations.

Endowments for departmental research may wisely range through the permutations of six figures into those of seven.

3. *Endowments for Special Research-combinations.*—The early settlers of the broad fields of the interior were accustomed to leave a wide unbroken 'turn-row' along their line fences, and long after the general settlement of the country, these presented almost the only remaining virgin soil. It has been much so in the pioneer cultivation of the scientific fields. Between the recognized realms of physics, chemistry, astronomy, biology, geology and other sciences there is a border-ground which has been less cultivated than these recognized fields, and here lie the richest virgin grounds of the scientific domain. Their adequate culture requires the cooperation of men trained in the several cognate fields. A combination of men skilled in physics, astronomy and mathematics is essential to the highest results in astrophysics. An association of men skilled in chemistry, physics, mathematics and geology is requisite to the most promising attack upon the complicated problems of geophysics, and so of other border-grounds. There is, therefore, an eminent opportunity to promote research by endowments which shall provide for the cooperative investigation of two, three or more men whose combined talents and training may fit them to engage jointly in a common inquiry. The endowment here must be large to be effective, but where it can be made adequate, its promise of fruitage is most eminent.

4. *Endowments for Schools or Colleges of Research.*—This is but a larger phase of endowments for departmental research, but with this difference: in the latter it is presumed that the departmental staffs of universities will continue to be, as at present organized, primarily for instructional work, while in the proposed endowment of schools or colleges of research it is presumed that research will be the dominant feature and instructional work will be incidental. Very great creative results would flow from the judicious establish-

ment of schools of research in chemistry, physics, biology, geology and other sciences analogous to the staffs of astronomical observatories where investigative work is now the declared purpose.

In the initial stages of the development of this scheme it is assumed that these schools of research must be individual and localized in different institutions, since no institution does now, or probably can in the immediate future, command the means for the establishment of such schools in all the departments that invite investigation. But if one institution were to concentrate upon one science or one limited group of sciences, and another institution upon another science or limited group of sciences, the universities of the country might *together* cover the field effectively. It were much better, to my mind, if the aspirations of a university should take a definite specialized form of this kind in some one or some few lines, than that it should distribute its effort over all lines with inferior success in each because of its limitation in men and means. It is probably not beyond the resources of any great university to secure the development of some one or two effective schools of research if it were content to make a selective effort and were wise enough to do this.

5. *The Evolution of Universities into Assemblages of Research-schools.*—Ultimately it is to be hoped that each of the greater universities will succeed in developing a large group of research schools, and that with this there will come a gradual reorganization of the constitution of universities, involving their transition from the function of personal education to the higher function of creative work. The English universities are now essentially aggregates of colleges, each of which is mainly devoted to personal education. The ideal of a university, as here entertained, would make the coming university an association of colleges of research for the benefit of mankind as a whole. In the English university, the several colleges cover essentially the same ground and are duplicative in their work and competitive in their relations. In the ideal university the colleges would occupy distinctive fields and be supplementary and stimulative toward each other and in no sense duplicative. Their primary function would be creative work for all mankind rather than didactic or disciplinary work for individuals.

6. *Endowments for Independent Institutions of Research.*—The preceding discussion has related essentially to universities. Aside from organizations under governmental patronage, universities are at present the chief agencies of research. It is doubtless quite within the truth to place to their credit by far the largest amount of creative work done independently of government patronage. None the less there is, to my view, a large and special place for independent institutions of research and endowments for such independent institutions are invited by their promise of fruitfulness. Every university has its special relations with some portion of the social organism by which it is fostered, and to which, because of that fostering, it is in special bonds. While this relationship of support and consequent bondage is one of greatly preponderating good, it is not without its moiety of trammeling and limitation. In order to fill out the full complement of institutions suited to the most effective promotion of creative work, a class of institutions not subject to these relations is needed. These needed institutions might, indeed, likewise have their own special relationships with their own limitations and trammelings, but they should none the less fill a place not occupied by existing universities, nor likely to be occupied by them. More than this, these

independent institutions of research should stand in relations of wholesome competition to the universities, and by representing a different phase of endeavor should thereby contribute to the broadening of the sum total of influences at work for the promotion of research.

7. *Endowments for the Higher Coordination of Research.*—Whatever may be the development in any of the institutions to which the phenomenal generosity of American men of means has contributed, or may yet contribute, it must still remain true that for an indefinite time the whole field of research can not be effectively cultivated by any institution, and there must be large need for adjustment and cooperation, that the energies of research may be distributed to the greatest advantage. Even if it were possible for any institution reasonably to attempt the whole field, cooperation and coordination in research should be cultivated to prevent wastage by unnecessary duplication and to give the greatest and best results by the adjustment of work to work. At the present stage of development, provision for cooperation is eminently desirable and endowments devoted to this end give promise of being preeminently productive. The ideal scheme of coordination contemplates the correlation of talent and equipment in all the institutions devoted to research without regard to institutional relations. It should be as free as human nature may permit it to be from predilection toward one form of institutional organization rather than another. It should be its function to develop, to use and to coordinate talent, effort and equipment wherever it may be, quite regardless of institutional connection, or locality, or of other relations than its possibilities of fruitfulness in creative work.

It need not be remarked here that we seem to be on the threshold of this great realization, and it is not too much to hope that each of the other forms of endowment will, in some large measure, appeal to the phenomenal generosity and appreciation of American men of wealth.

T. C. Chamberlin.

I will confine my remarks to the question as to the most effective use of endowment for the publication of scientific work.

While in Europe, particularly on the continent, we find numerous publishers who undertake the publication of scientific works and periodicals as business ventures, there is no such publication known in our country, except in the applied sciences or in so far as books can be used as text-books. All work in pure science that is published in our country is published by the help of edowment of one kind or another.

It is not quite easy to determine the reason that has led to this state of affairs. It may be due partly to the newness of science in America, partly to the great cost of printing, and partly to the limited number of buyers of scientific books; but it seems also probable that the vast amount of scientific publication carried on by our government, and the lavish distribution of its publications have discouraged private enterprise.

The United States government and the state governments are the most liberal supporters of scientific publication. Next in importance are the scientific societies which are distributed all over our country. Third in order are universities and other institutions of learning; and, finally, wealthy friends of science. The total amount of money invested annually in scientific publications is quite considerable. A comparatively insignificant part only comes from that part of the public which purchases scientific publications on account of their contents. From an economical point of view this is an abnormal condition; and the question arises, whether

the method of scientific publication can not be so improved as to make the available funds more effective, and to secure the support of libraries and of private individuals who make use of scientific publications.

Setting aside the publications supported by the government, scientific publications may be roughly divided into journals devoted to special branches of science, special memoirs supported by scientific societies or institutions, and miscellaneous serials supported by general societies, such as academies of sciences and museums. Among these the only class that is entirely, or at least almost, self-supporting, are the special technical journals, which appeal to a well-defined group of people that constitute a society supporting the journal, or which are published by a few individuals and pay the cost of manufacture through subscriptions. The second and third classes of publications are almost entirely supported by voluntary contributions, not by the public that makes use of them. If it were feasible to readjust the conditions of publication in such a manner as to create a market for the publications here referred to, the facilities for publication would be materially increased, the available endowment would be made more effective, and the claims for more liberal financial support would become justifiable.

It seems to my mind that the traditional policy of societies and other scientific institutions to publish serials devoted to miscellaneous subjects is, to a great extent, the cause of present conditions, and that by proper cooperation between such societies and institutions many of the difficulties under which we are laboring might be obviated. At the present time numerous academies of science publish volumes of transactions, proceedings, annals, etc. Most of these publications are not strong enough to command the support of the scientific public. They find their way into libraries of other societies by exchange. They are sent to the members of the society that publishes the serial, but they are not read by them on account of the miscellaneous character of the publication. For this reason the serials of most of our mixed societies have come to be an excellent means of burying good scientific material. They are not read; they are placed on the shelves of the libraries of societies, which, on the whole, are unable to make their books accessible to the reading public. There are, therefore, two points of view from which the present method of publication may well be criticised. The one is that the material is combined into volumes in such a way that an exceedingly small part of each volume only is useful to the student of a particular branch of science. The second is that these serials find their place, not in important libraries, but rather in small libraries of societies, where it is very difficult to consult books.

This method of distribution is also a survival of conditions which may have been desirable in former times, when there were no great public libraries, and when the scientific society had to perform general educational functions, among them that of maintaining a reference library. At the present time this need is well taken care of by various kinds of public libraries, so that it may well be doubted whether, at least in most of our larger cities, it is worth while for societies to continue the accumulation of books. As a matter of fact, the method of building up libraries by means of exchanges is one that does not seem to fit well into our economic conditions. In all other walks of life the acknowledged medium of exchange is money, and we measure the equivalent of an exchange by this standard. In building up

society libraries we are content with spending money on publications and taking in exchange for them, not what we want, but whatever we happen to get. In consequence of this, the editions of society publications are too large, and the libraries which are accumulated are of little practical use.

These conditions might easily be remedied by a proper cooperation between the societies and institutions of our country. If the total output of scientific matter produced by our smaller mixed societies could be combined and arranged in serials, each covering one of the important branches of science, it would be possible to provide a number of scientific serials, each of which would be of sufficient importance to command the attention of scientists, and which for this reason would have to be taken by some group of men who subscribe for the special journals, and also by all the larger libraries of our country and abroad. A demand for these publications would, therefore, at once be created, and the material that is now difficult of access would become available to all. The society funds devoted to publication would be relieved by the increased subscriptions, and the publication work would, therefore, become much more nearly self-supporting than it is at the present time.

At present the mode of bookkeeping of scientific societies is such that the charges for publication and for accessions to the library are not separated. This is due to the fact that the accessions to the library are paid by exchanges. If in the bookkeeping of societies these two items were clearly separated, it would be found that the amount of money invested annually in the library is entirely out of keeping with the usefulness of the library, except in the few cases of old societies which own houses and sufficient funds for the proper administration of books. In all other cases it would seem more advantageous to discontinue the accumulation of books, and leave this branch of work to public libraries.

The only difficulty in organizing the work of publication in this manner lies in the reluctance of societies to lose any of their individuality and to become closely associated with a larger body; but scientific societies and institutions should recognize the value of cooperation and the fact that the advance of science will be best promoted, not by selfish endeavor to aggrandize each society, but by willing association with others and by cooperation towards a common goal.

If the publication work of societies and institutions were organized in this manner, the serials would, to a greater or less extent, be similar in form to the scientific journals described before. On the whole, it may be said that these journals offer an adequate means for the publication of short papers. It would, therefore, seem appropriate for the mixed societies to devote their energies rather to the publication of memoirs that are too extensive for the journals. Such series of memoirs might well be made supplementary volumes of the special journals, and in this way a unification of the whole subject-matter devoted to a certain branch of science might be brought about.

There is another field in which cooperation might result in much more satisfactory arrangements for the advancement of science than those we have at the present time. Various journals and society publications print in each number a selection of notes, bibliographies, reviews, etc., which to a considerable extent overlap. The preparation of reviews is more or less left to chance, and the attempts at systematic collection of bibliographies are few. Nevertheless, we all recognize that systematic reviews and bibliographies are sorely

needed by students. It might seem that through cooperation of technical and mixed societies such bibliographies might well be prepared, and that in connection with their serials each group of societies might undertake the preparation of a *Centralblatt*.

These considerations make it clear that a vast saving of money and energy may be effected by the proper coordination of the work of mixed societies, and that the publications may be made infinitely more effective. Such coordination would require a certain sacrifice of independence on the part of each society, which would be amply repaid by the greater usefulness of its work. I think, if we succeed in moving on in the direction of thus centralizing effort in every branch of science, we shall be justified in asking for more liberal support of our work, not so much by endowment as by enlisting the interest of friends of education who will support the work of libraries by subscribing to scientific serials. If all the libraries in our country that have more than ten thousand volumes were in a position to subscribe to the organized scientific periodical literature, there would be no difficulty in providing for the publication of the bulk of all scientific matter worthy of publication, and without asking for heavy endowments. There would still remain a number of special and costly works which societies and institutions could not well support out of their own funds. For such publications we might justly ask the assistance of wealthy friends of science. For these we should try to obtain sufficient endowment, which might be allotted by a national council representing the various branches of science.

I thoroughly believe that our first duty is to systematize our efforts, and to economize by such systematization. In this way we shall make the available funds go much further than they have ever gone before. We shall make our work more effective, and enlist the cooperation of the reading public, and we shall be in a position to ask with greater authority for the support of publications that neither the government, nor societies, nor the reading public is able to support.

FRANZ BOAS.

ON the principle that it is sometimes important to know what not to do, I would offer a few remarks on one of the practical aspects of the inverted question: How are we at present using endowments ineffectually in scientific research?

To those who see our fires of learning gleaming only from afar, and are not near enough to see all the smoke, many of our university customs must appear to be enveloped in a haze of sanctity. One of these is the esoteric custom of awarding fellowships for research. In the language of the apiculturist, a university fellowship seems to the uninitiated like a rich mass of 'royal jelly,' to be fed to some fortunate but impecunious larval investigator for the purpose of enabling him to develop into a leader in the hive of scientific workers. And very often the larval investigator is of the same opinion till the food is administered and he suffers the disillusionment of the initiated.

He is no sooner awarded a fellowship of a few hundred dollars—and this is true even in our most richly endowed universities—than he finds that half or the greater portion reverts to his benefactor for tuition and laboratory fees. Nor is he even then permitted to settle down to his work in peace with the small pecuniary remnant and the 'honor' thereto appertaining. He may find that the richer the institution, the more it has need of his services as an instructor or laboratory assistant. This is because the university has

committed itself to what for want of a better term I shall call the 'lunch-counter' policy of perpetually offering new courses and subjects—not so much, perhaps, for the purpose of keeping pace with the multiplying and advancing sciences, as for the purpose of keeping itself constantly before the eyes of the public. One heavily endowed institution is known to have utilized its fellows as attendants in the departmental libraries, because the expensive 'lunch-counter' policy would not enable it to pay the salary of departmental librarians. And it is not impossible that somewhere between Maine and California there may be universities so heavily endowed that they can require their fellows to perform regular janitorial services.

With the insignificant remnant of his fellowship and such scattered remains of his faculties as can be scraped together from the more or less perfunctory study of one or two 'minor' subjects, and from the neglect of his duties as a sort of poor relation in the university household, the fellow is supposed to be 'doing original work,' 'making researches,' 'investigating.' If he was a child with strong investigating impulses, like all normal children, and has retained a shred of these ancient and pithecoid, but nevertheless divine, impulses after running the gauntlet of some of our secondary schools and colleges, he is expected, under the limitations afore-mentioned, and while eating any thing and living in any way, to produce some epoch-making work *ad majorem universitatis gloriam.*

As a matter of fact, the poor fellow—and he is, indeed, a poor fellow—is given some problem which to the body of his chosen science bears about the same proportion as a single nucleus to the whole human body. He proceeds to collate all that Schultze, Mueller, Schmitt & Co. have written on the subject, glues it together with a little of the secretion from his own larval sericteries, and prepares his jaded nerve-centers for the final examination farce. His professors assemble, and, bereft of all sense of humor, instead of smiling at one another like a troop of Roman augurs, sit through the farce with faces as long and as blank as the windows of the favorite-imported-imitation-Gothic-university-architecture—that gingerbread relict of church-ridden mediævalism—till the candidate wriggles through with a *rite* or a *cum laude,* or perchance, if he has been sufficiently intrepid to mount to sources unknown to his professors, with a *summâ cum laude.* And the newly-fledged doctor goes forth into the country to start a fresh center of mental infection of the same old type.

It would, indeed, be difficult to devise a more effectual method of hampering research than by the petty restrictions placed on fellows in most of our universities. Such, among others, are the pusillanimous objections to permitting fellows to work *in absentiâ* or where they can best obtain their materials, consult the best libraries and museums, work in the necessary marine laboratories, botanical gardens, etc. The results of these restrictions, so far as American biology is concerned, are only too apparent in the monotonous output, the few lines of investigation that are being intensively cultivated, and in the not infrequent cases of intellectual parasitism and commensalism, not only on the part of the students—that is to be expected—but between professors of different institutions. In the meantime the whole of tropical America, as well as the tropics of the Old World, abounding in materials of the greatest interest, not only to the botanist and the zoologist, but to the paleontologist, geologist, mineralogist, archeologist, anthropologist and pathologist, are being rapidly opened up to us. For any adequate utilization of such magnificent opportunities,

and in view of the fact that our gilded youth show little inclination to indulge in anything so tame as research, we must have fellowships of some kind, but fellowships with no niggardly academic restrictions. Our young men should be enabled to spend months or even years in localities where they can study organisms in their natural environment. The prevalent type of university expedition, that hurries through a country, collects a few miscellaneous specimens and observations and makes for home, may be better than nothing, but it leaves us little the wiser concerning the most important problems presented by the fauna and flora of foreign countries. Stuffing our museums with specimens is not necessarily advancing biological science. And it is not even necessary to call attention to the tropics in this connection. Vast stretches of our own country are all but unknown biologically, and are liable to remain so as long as our graduate students and fellows are persuaded that the salvation of the science depends on their becoming sessile organisms with *idées fixes* on the twinkling of the centrosomes, the twiddling of the chromosomes and a few other matters of similar import.

I am aware that much of what I have said may belong to past history, and may even apply to men of straw, but there is still a good deal of old straw, or what the Germans call 'Zopf,' in all our universities. Some of this is undoubtedly of our own cultivation, but much of it has come to us in the packing boxes with intellectual commodities from Europe. The sooner we set fire to it the better.

I would venture, in conclusion, to advance the following suggestions as a remedy for some of the evils connected with our fellowships:

1. Let us select as fellows only those young men who have well-developed investigating instincts and the proper preparation, maturity and mental balance to apply themselves perseveringly to the business of research.

2. Let these young men be given sufficient monetary aid to detach themselves from an inadequate environment and to do their work wherever they can find the best facilities, in America or in any other portion of the habitable globe.

3. Let us understand that a fellow is not a recipient of alms, and that he is not only honored by the university, but confers an honor on any institution with which he may become connected in the capacity of investigator.

4. Let us have sufficient knowledge of human nature and the historical development of the sciences not to expect immediate and inordinate scientific returns for any pecuniary aid which we may be able to bestow.

WILLIAM MORTON WHEELER.

IT is very gratifying to note the constantly growing interest in scientific investigation in all parts of America. To this many agencies have contributed. Among them the universities and experiment stations of the government, both national and state, naturally come into the mind as the principal institutions by which research has been fostered. Endowments placed in their hands have been in almost every case wisely administered. Research, however, has not, in any American university of the first rank, been definitely put forward as of primary importance. It has been compelled to conquer a place for itself and show its right to consideration under the university organization. I am not sure that in some instances research in the universities has not been something of a 'fad.' It has been sometimes put before immature and naturally incapable persons as the only goal for their

endeavors. As a result considerable misdirected effort has no doubt been made, and in a certain sense it is to be deplored that the grade of investigation in American institutions has not been higher than it is. The important point is, however, that the universities and colleges have felt the necessity of going beyond the work of formal instruction, and time may be expected to correct the errors and inadequacies of their efforts in investigation.

I view with some degree of pessimism all suggestions concerning cooperation in research between different institutions. I do not mean by this that no cooperation is possible, but in many instances cooperation means subordination, and is one step in the decline of institutional research. It seems to me that institutions are very much like individuals, and that the important thing is to have as many as possible of them take up the work of research and carry it forward to the best of their ability. The principle of natural selection will work among them towards the elimination of the weaker and unfit. It does not seem to me that institutions for research or avenues of investigation have been so greatly multiplied in the United States that the time has now come for combining or 'mergerizing' them. On the contrary, one should view with enthusiasm the addition of new institutions and foundations by means of which scientific investigation may be more universally developed throughout the country. The situation in America is such, if I read it right, that *results* of research in the discovery of the laws and forces of matter are, after all, not so essential as the *spirit* of research disseminated throughout the nation.

It would seem that in society there is some automatic mechanism at work by means of which emergencies, as they arise, are rightly met. There comes a man for every hour, whether its burden be great or small. Generally speaking, I believe that research in America has developed naturally and reasonably to its present respectable proportions. The great foundations, whether they be universities or learned societies, private or governmental, have shown the higher social wisdom and have done their part, unconsciously perhaps, but none the less excellently, in the development of true science and genuine research. Boards of directors may generally be trusted to use endowments up to the level of the intelligence of their community and generation.

I favor the multiplication of institutions and agencies for research. They should be untrammeled and free to work out their own destiny. Every new foundation should be welcomed and should be permitted to stand or fall as it may show strength or weakness. Experience will be the only teacher of its board of directors and results the only criterion of their success in administration.

Conway MacMillan.

The president of the Society of Naturalists has kindly invited me to represent the psychologists in our discussion. I suppose his idea was that I should formulate some endowable plans for psychological researches. But that has been done, perhaps even overdone; modest and luxurious, possible and fantastic plans have been outlined, sufficient for the psychologists of the whole century, and I have had my full share in it—I felt unwilling to fish in those waters once more. And yet that was the thing to do if I understood and interpreted our president correctly; there was only one chance for me: I might try to misinterpret and to misunderstand his invitation and with this intention I accepted it.

I thus misunderstood my task to mean that I ought to consider the whole problem of research and endowment as a psy-

chological phenomenon, and I can not help it if my psychology has even a certain national flavor; the president knew that my views as well as my English are 'made in Germany.'

I, for one, indeed, believe that, if the improvement of scientific research in America is under discussion, the psychological factor which is involved can not be emphasized too strongly; it is the *psyche* of men and not the physical apparatus that determines the value of research. I know very well that work in a rich laboratory is much more comfortable than in a poor one, but the ultimate productiveness of the research does not depend upon it. Wherever good productive work is done, there is a strong moral claim for greater comfort in work, for greater leisure, for finer devices to make work more effective, more elegant, more complete, but the essentials of research are not touched by these factors. In the sphere of research, as in all spheres of life, there is a dangerous temptation to take greater comfort in itself for greater culture and internal progress. The whole history of science suggests the opposite. Everywhere may we see that the decisive discoveries and experiments were made with modest means and clumsy apparatus, and the change from poverty toward luxury has not seldom meant a change from concentration to superficial expansion. The great Helmholtz once said to me: "In the small laboratories with home-made apparatus they mine gold, while in the large and rich ones they transform the gold nuggets into sounding brasses."

How little important is the equipment is shown just by the situation in this country. It is an insufficient excuse for unproductiveness if the fault is laid to the defective equipments. If the outfit and the means were the determining factors we Americans should be far ahead of European research in many fields. In my own science, experimental psychology, the commercial value of the equipment of the existing psychological laboratories in America is perhaps five times greater than that of all German, yes, of all European, laboratories, but it would be absurd to say that we have really done five times more than those on the other side of the water. The real foundations of our science were laid by Professor Wundt in a German laboratory whose equipment is surpassed to-day by many frontier colleges in this country. And I deny that my science is an exception. In zoology, for instance, there are small colleges here whose names are hardly known, whose equipment surpasses that of universities like Leipzig, and yet, as the poet says, what difference to me!

The only two factors which really count for research are to be found in the minds of the men; they are, first, intellectual quality, and secondly, the will to achieve. These are the two respects in which American research is defective. Have we the right kind of man behind the gun?

The psychologist must ask, of course, in general: Is the right kind of research man to be found at all among this people? I should say: Certainly—perhaps nowhere a more ideal combination of the right features. Quick, sharp grasp of a situation, brilliant inventiveness, persistent energy, talent for organization, unselfish idealism, all these are characteristics of the best type of American and exactly these are the conditions of successful research. The misery of the whole situation is that if we abstract from the numerous exceptions and look on the broad average, this right type of man sits in the counting-houses and law offices, is busy in commerce and industry and politics and medicine and what not—but into the graduate schools and into college work there rushes, together with

some excellent men, the swarm of good modest fellows without much ambition and talent—and rushing is still too energetic a word; that type of man does not rush, but is passively moved forward. Everywhere in this country the average graduate student who prepares for academic work represents as to intellectual energy a lower type of man than the average undergraduate, and yet with second-rate men there can not be a first-rate science, even if a billionaire comes to them, as Jupiter came to Danæ, in the form of a shower of gold.

But why do our finest men shrink away from the career of the scholar, which is sought, in Germany for instance, by the very best: simply because it does not sufficiently stimulate their ambition. There are not the great premiums at the top, and it is well known that in the eyes of youth every career gets its social value just from chief premiums and exceptional gains. The career ranks here with the humble one of the schoolmaster; how can it stand in competition with banking and law? Thus I should say to those who have millions ready for endowment: first make the career attractive, so that it can tempt more men of the first-class type; create great premiums by putting above our present university system a still higher institution, an over-university where the finest masters of research in every field, chosen by their peers, are brought together for far-reaching work which transcends the possibilities of the educational institutions. Whatever you can do to give to the career national glory thus to attract the finest men, will be productive for the work of research.

But I add a second point. From one motive or another a lot of fine men enter the career already to-day, and yet even they do not live up to their best. As I said in the beginning, it is not only a question of capacity, but one of concentrated will to achieve. Even our good men, while they start with high intentions, too often give up after a short beginning; the spirit of research evaporates and the routine teacher remains. I know that there are plenty of external reasons, too many are overburdened—and yet, the chief reason is again a psychological one; there is no stimulus for productive scholarship. They feel too soon that such achievement does not count for their career, yes, that they have to stand below the man who spends his time in mere teaching and administration. Make the academic career in the real universities, the promotion to higher positions, dependent in first line upon research work, as it is in Germany, and the work will be done, in spite of all obstacles. There is at present no greater educational need in this country than to educate the trustees of the universities. Everywhere, with a few exceptions, the universities are still administered after the pattern of college administration, and the research spirit is thus artificially suppressed. Let the trustees understand that research can grow only where it is considered as the backbone of university life, as the condition for appointments, and that only scholars, not laymen, can be judges for it; then we shall have research which will keep pace with the marvelous progress of this country. And to my billionaires I should say: Help us to bring about this change; endow with your treasures those existing universities which show the right appreciation for productive work.

I have so far expressed dissatisfaction with the existing conditions; are there no psychological conditions which are favorable here for scholarly work? Certainly, above all, one most important one—the spirit of helpfulness and ambition in the special academic communities, the readiness of the alumni to stand for their Alma

Mater, to push forward her work and her equipment, to make it strong in the rivalry of the various institutions. This local academic pride has secured the progress which the last decades have seen: here lies the one great advantage which the American institutions have over the German universities, which have to await everything from the governmental center. Thus, if we desire that this noble progress shall go on, by all means do not tamper with this local self-activity by giving favors on application. Do not undermine it by central interference; do not annihilate that feeling of responsibility among the alumni by playing providence. Your good-will would be merely an opiate for the energies of the communities; they would soon leave the whole care to you, and matters would then be worse than before. The only help for the individual researcher which is not to paralyze the eagerness of the community, must come either through the medium of the institution to which he belongs, or from central establishments which shall benefit all alike. For instance, we badly need large printing houses which shall print scientific matter for every one without profit, or mechanical establishments for cheaper apparatus, as in such respects we are really worse situated than Europe.

Furthermore, a patchwork of scattered favors will not only ruin those forces which work for good to-day, the enthusiasm of the alumni, but it will be harmful even to the workers themselves. Firstly, it introduces a central power without self-government: we may have the most ideal men in control, and yet the door would be wide open for all the bad features of the spoils system and favoritism, because in questions of research the decisions must remain dependent upon the prejudices of scientific cliques and schools. We do not want academic party machines and party bosses; we do not want wire-pulling for one scientific school as against another; there are too many alarming reports afloat already. There may be discussion whether state life prospers better with self-government or with paternal autocracy, but there can be no discussion which of the two systems is the better for research and scholarship. Research needs free competition.

But worse than the absolutely unavoidable arbitrariness of the distribution must be the moral effect of the system on the researchers who are favored. The charity system is nowhere more tempting, but just, therefore, nowhere more ruinous than in the republic of scholars. Charity hides the problem but can not solve it. Alms for research, tendered on application with pledges for good behavior and typewritten manuscript, will do what the economists everywhere find as the results of mere charity. Instead of building up the community it will weaken it. Charity is everywhere the easiest way out of a difficulty, because it leaves the real difficulties to those who come later. In the first moment you hear a thousand God-bless-you's and after a little while the energies are emasculated. Research ought not to go begging, research wishes to be free; research wants respect, not clemency; its rights, not favors. Research desires the improvement to come from within, not from without; by applying endowments not according to the principles of politics, but according to the principles of psychology, trying to raise the average type, trying to stimulate everybody to his best work, and trying to create better conditions for all alike; in all these three respects endowments might work wonders.

HUGO MÜNSTERBERG.

HARVARD UNIVERSITY.

SCIENTIFIC BOOKS.

The Influence of Light and Darkness upon Growth and Development. By Daniel Trembly MacDougal, Ph.D. Published by the aid of the David Lydig fund bequeathed by Charles P. Daly. Memoirs of the New York Botanical Garden, Volume II. New York, 1903. (Issued January 20, 1903.) Large 8vo. Pp. xiv + 319.

This notable contribution opens with a short historical account (34 pages) of investigations bearing upon the subject under consideration, the earliest of which was that of John Ray in 1686, followed in 1727 by Hales, in 1754 by Bonnet, in 1776 by Mees, in 1782 by Senebier, and in 1783 by Tessier. This brings the record down to the opening of the nineteenth century, in which we find seventeen names in the first half, among which are DeSaussure (1804), DeCandolle (1806), Davy (1815), Knight (1841), Payer (1842) and Draper (1844). Shortly after the middle of the century Sachs began his work (1859), and continuing for thirty years or more, added 'an enormous number of facts concerning growth and the relations of light to plants,' and 'led the way to nearly all of the modern work upon this subject.' No doubt he revolutionized this part of the science of plant physiology, and yet it is a curious fact, as Dr. MacDougal remarks, that 'scarcely a single one of his conclusions concerning etiolation and the influence of light upon growth is tenable at the present time except in modified form.' Stimulated by the work of Sachs, a host of investigators sprang up, their number increasing rapidly during the closing quarter of the century. The latest names recorded are those of Ricome and Noll (both in 1902), immediately preceded by Livingston, Goff, Wiesner, Nabowick, Neljubow and Schulz (all in 1901).

The body of the book (166 pages) is made up of careful records of observations upon ninety-seven different species of spermatophytes and pteridophytes. Here, as one reads page after page of observations, it is made very plain that this portion of the book required a deal of patient work. By means of drawings made from photographs the text is helped out and made much plainer than would have been possible otherwise. The earlier work was done by the use of small portable dark-chambers of zinc or wood, but after the removal of the author to the New York Botanical Gardens in 1899 a specially constructed room in the middle of the museum building was used. Here the temperature was very constant, and a normal atmosphere was secured by proper ventilating apparatus. Entrance to the room was through double doors with a vestibule between, and the plants under observation were examined by means of a single-candle or a four-candle power electric lamp.

As a matter of course, the most obvious result of the growth of plants in total darkness, aside from the loss of green color, is the more or less complete suppression of the leaf-blades and an elongation of the internodes of the stems. There are also marked changes, which, however, are not readily seen, in the minute anatomy of the leaf and stem, as are well shown by many excellent figures. In some cases the histological changes are quite remarkable, as in the leaf of *Oxalis lasiandra*, the stems of *Galium circæzans* and a seedling oak. The epidermis of *Opuntia opuntia* shows very striking differences, as is the case to a less degree in most plants observed. Here, however, while the epidermal cells are usually much modified, the stomata themselves are but little changed on the etiolated leaves. Among the interesting modifications figured, none are more so than those of the 'pitcher leaves' of species of *Sarracenia*.

In a discussion of the effect upon the plant of prolonged or continuous light, the author concludes that "increase in the total duration of illumination to which a plant is exposed, during its vegetative period, either by artificial nocturnal illumination, or by cultivation in Arctic regions, results simply in a correspondent acceleration of the seasonal development of the plant, by which a greater amount of work is accomplished within a given number of days. The extinction of the daily 'resting period' brings no distinct reaction so far as important anatomical features are concerned, although an exaggerated produc-

tion of certain substances is found to take place. Neither is any retarding or paratonic effect to be seen as a result of this continuous illumination."

The chapter on the theories as to the nature of etiolation is curious and interesting. The theories of the earlier investigators quite naturally were very crude, and it is not too much to say that something of this crudity continues even to the present day. Apparently we are not yet ready to formulate a satisfactory explanation of the action of a plant when grown in darkness. The 'adaptive theory' of Boehm (1886) as elaborated by Godlewsky (1889) appears to be the one most favored by botanists just now. It interprets etiolation as a direct adaptation, and assumes that 'the attenuation or elongation of axial organs is a means of lifting chlorophyll-bearing organs past theoretical obstructions.' Of this the author says, however, that 'the forms presented by the shoots of a greater majority of the species examined do not exhibit any beneficial adjustments by which the plant might free itself from encompassing darkness, and lift its leaves and reproductive organs past the obstruction that intercepts the rays.'

We should like to refer to the chemical composition of etiolated plants, the rate and mode of growth of such plants, the stimulative influence of light, etc., taken up in this most interesting book, but space forbids further discussion at this time. The author is to be congratulated upon having added so valuable a book to the growing list of his publications.

CHARLES E. BESSEY.
THE UNIVERSITY OF NEBRASKA.

International Catalogue of Scientific Literature, first annual issue, R—Bacteriology. Published for the International Council by the Royal Society of London. London, Harrison and Sons, 45 St. Martin's Lane. Vol. VIII. December, 1902. Pp. xiv + 314.

This is a comprehensive bibliography of books and other contributions to bacteriological literature which appeared during 1901. Author and subject lists are given, the latter arranged according to a decimal system. The plan and essential features of this undertaking, which is an outgrowth of the 'Catalogue of Scientific Papers' formerly published by the Royal Society of London, have already been described in SCIENCE, N. S., Vol. XIV., p. 861. In the present issue there are 2,206 titles indexed under the authors index and presumably the same are grouped under the subject classes. To cover the literature of any scientific subject in all languages is a stupendous task, and for those who use the catalogue it will doubtless be found a valuable aid. While it is always easier to criticise than to construct, yet there are certain features of the present volume to which attention deserves to be called.

Exception could be taken to the admission of articles on malaria, Texas fever, surra, fungus and nematode diseases, etc., as well as many other titles that are even more remotely connected with the subject of bacteriology, but particular attention at this time is desired to a single feature.

One at all acquainted with the literature of the subject is at once struck by the paucity of reference to articles published in the United States. A careful examination of the contents of the volume revealed but about eighty titles of books, addresses, magazine articles, etc., nineteen different periodicals being represented in the list. The *Journal of the Boston Society of Medical Sciences* is first in the list with fifteen references, followed by *Popular Science Monthly* with nine, the *Journal of the American Medical Association* with six, and the *Philadelphia Medical Journal* with the same number, the others having from four to a single title indexed. None of the publications of the U. S. Department of Agriculture nor of any of the Experiment Stations are mentioned, although during 1901 there were published from these institutions many articles relating to the bacteriology of plant diseases, dairying and veterinary science. For the sake of confirming the catalogue references a number of publications of 1901 were examined to see if they were prop-

erly represented. The *Journal of Comparative Medicine and Veterinary Archives* is mentioned but once in the catalogue, although there were eighteen leading articles in which bacteria were described as the cause of the disease mentioned, and in some cases extensive studies were given of the diagnostic and cultural characters of the organisms. In the *American Veterinary Review,* not noticed in the catalogue, there appeared sixteen original articles similar to the above. In the *Medical Dial,* also not noticed, were nine leading articles treating of bacteriological studies, diagnoses and bacterial investigations of water supplies, milk, etc. The *Medical Record,* to which there appear five references for the whole year, contained in the issues from June to December, twenty-two articles that one would have expected to have found mentioned.

Since this publication, having an American representative, shows such an inadequate representation of American literature, it can hardly be wondered that so many European investigators not having access to the original publications are unacquainted with what is done on this side. Omissions from the present volume are to be included in the next, according to a note in the catalogue, and it is to be sincerely hoped that a greater effort will be made to fairly represent our American scientific literature. Walter H. Evans.

Washington, D. C.

BOURNE'S COMPARATIVE ANATOMY OF ANIMALS.*

The first volume of the two comprising this work has already been noticed in this journal (Science, Vol. XII., p. 311, 1900). The present volume consists of a series of somewhat detailed descriptions of the structure and ontogeny of selected types of animals, the whole being intended to fit students for the preliminary and intermediate examinations in the British universities. The animals selected are the liverfluke (how this is cœlomate does not appear), earthworm, fresh-water mussel, snail, *Apus, Astacus,* cockroach, *Amphioxus,* dogfish, frog, with a chapter on other annelids and a final one on the mammals.

As a whole, the descriptions are clear and accurate, and the seventy-seven illustrations illustrative of the text. Particularly instructive is the cut (fig. 44) of the pharyngeal region of *Amphioxus.* However, it is not well adapted for use in American schools, for it tells the student just those points which we insist that he shall ascertain for himself, so far as possible, from the specimen. As a 'cram manual' it would have a value. Lastly, the title is misleading. The whole work is descriptive, not comparative; in fact comparisons and broader features are rare in this second part, which in many respects falls short of the first volume.

J. S. Kingsley.

* 'An Introduction to the Study of the Comparative Anatomy of Animals,' by Gilbert C. Bourne. Vol. II., 'The Cœlomate Metazoa.' London, George Bell and Sons, 1902, pp. xv + 321. 4*s.* 6*d.*

SCIENTIFIC JOURNALS AND ARTICLES.

The March number (Volume 9, No. 6) of the *Bulletin* of the American Mathematical Society contains: Report of the ninth annual meeting of the American Mathematical Society, by Professor F. N. Cole; Report of the December meeting of the San Francisco Section, by Professor G. A. Miller; 'The abstract group G simply isomorphic with the alternating group on six letters,' by Professor L. E. Dickson; 'Note on a property of the conic sections,' by Professor H. F. Blichfeldt; 'The analytic theory of displacements,' by Mr. R. W. H. T. Hudson; Notes; New publications. The April number of the *Bulletin* contains: Report of the January meeting of the Chicago section, by Professor T. F. Holgate; 'Some groups in logic,' by Professor E. W. Davis; 'Cesàro's Intrinsic Geometry,' by Dr. Virgil Snyder; 'Gauss's Collected Works,' by Professor James Pierpont; 'Analytic projective geometry,' by Dr. E. B. Wilson; Shorter notices; Notes; New publications.

SOCIETIES AND ACADEMIES.

PHILOSOPHICAL SOCIETY OF WASHINGTON.

The 562d meeting was held January 31, 1903.

Professor A. N. Skinner, of the Naval Observatory, spoke by invitation on the 'Prog-

ress of the Zone Observations of the Astronomische Gesellschaft.'

All determinations of the positions of the fixed stars through the time of Hevelius (1650) were made without the use of the telescope. The time of Flamsteed (1725) marks the first use of the telescope for this purpose. Bradley's star-place determinations (1750) are considered to be the beginning of the astronomy of precision. Among the astronomers who led in the determination of star places were: Lalande at Paris, 50,000 star places (1800); Piazzo at Palermo, 6,700 star places (1800); Rümker at Hamburg, 70,000 star places (1835); Lamont at Munich, 33,000 star places (1845).

At the suggestion of Bessel the Berlin Academy of Sciences (1825) undertook the publication of star charts to contain all stars visible with a 3-inch telescope to be executed by different observers and to cover the belt $-15°$ to $+15°$ of declination. To furnish a groundwork for these charts Bessel swept over this area $-15°$ to $+15°$ decl. in zones with a meridian instrument, and determined the places of a large number of stars for 1825. Later he extended his zones from $+15°$ to $+45°$ decl., embracing a total of 70,000 star places between $-15°$ and $+45°$ decl. Argelander, a pupil of Bessel, extended Bessel's zone determinations from $+45°$ to $+80°$ decl. at Bonn (1842) with 22,000 star places, and southward from $-15°$ to $-31°$ decl., also at Bonn (1850), with 17,000 star places.

Bessel expected that only a few years' time would be required to complete the Berlin Academy charts, but their execution dragged along until 1860, when at last they were finished.

Because the Berlin Academy charts, then incomplete, proved inadequate to satisfy the increasing needs of astronomy, Argelander, with his assistants at Bonn, in 1852 commenced the 'Great Bonner Sternverzeichniss' to give the approximate places of all stars visible with a 3-inch telescope from $-2°$ to $+90°$ declination, accompanied by an atlas of all the stars. This great work, comprising 324,198 stars in three quarto volumes with the atlas, was finished in 1860. In 1886 Argelander's successor at Bonn, Schönfeld, extended this great catalogue from $-2°$ to $-23°$ declination in a quarto volume containing 133,659 stars.

The Astronomische Gesellschaft, founded in 1865, planned the first division of their great zone catalogue to comprise accurate determinations of the places of all the stars to the 9.0 magnitude, inclusive, contained in the 'Bonner Sternverzeichniss' from $-2°$ to $+80°$ declination, the work to be distributed among different observatories in zones of about five degrees broad. The following observatories participated in this work: Kasan, Dorpat, Christiania, Helsingfors, Cambridge (U. S.), Bonn, Lund, Leiden, Cambridge (England), Berlin, Leipzig, Albany (U. S.), Nikolajew. This series of catalogues in fifteen volumes has been published except the Dorpat zone. These fifteen catalogues will contain about 137,000 star places for the epoch 1875.0.

Immediately on the appearance of the southern extension of the Bonner Sternverzeichniss' by Schönfeld in 1886, the Astronomische Gesellschaft arranged for the determination of all the stars to the 9.0 magnitude, inclusive, in Schönfeld from $-2°$ to $-23°$ declination. The following observatories agreed to participate in this work: Strassburg, $-2°$ to $-6°$ declination; Wein-Ottakring, $-6°$ to $-10°$; Cambridge (U. S.), $-10°$ to $-14°$; the Naval Observatory, Washington, D. C., $-14°$ to $-18°$; Algiers, $-18°$ to $-23°$.

Wein-Ottakring has published the journal of its zone observations $-6°$ to $-10°$; the Naval Observatory has published the journal for the zone $-14°$ to $-18°$ as Volume II. of its publications, second series, 525 pages quarto. The discussion of the results and the preparation of the catalogue from the Washington observations are in progress.

Mr. R. A. Harris then pointed out 'The Uses of a Drawing Board and Scales in Trigonometry and Navigation.' The object is to solve graphically spherical triangles. The apparatus consists of a board about 40 x 20 inches, with angular graduations on three sides radiating from the center of one long side, and of edge scales graduated to give

various functions of an angle or of half the angle, together with the usual T-square and straight edge. By these instruments a plane triangle is constructed having sides and angles proportional to the proper trigonometrical functions of three known parts of the spherical triangle to be solved; then the remaining parts may be scaled off.

The next paper was a report by Mr. F. G. Radelfinger 'On the Analytic Representation of Function.'

The general problem of obtaining an analytic development coextensive with the domain of existence of a function was stated by way of introduction; then the author reviewed the most important recent researches bearing on this general problem. He confined himself in the main to a synopsis of the results obtained by Mittag-Leffler in his four 'notes,' which have appeared at intervals during the last three years in his journal, the *Acta Mathematica*.

The analytic developments constructed by Mittag-Leffler are in the form of series n times infinite, which can be transformed into singly infinite series of rational polynomials; these developments converge within an extended region, which for $n = \infty$ coincides with a star introduced by him; the star embraces the whole plane excepting radial crosscuts extending from each singular point to infinity. Several methods of constructing these expressions have been made use of and expounded in the several notes. Borel's work was also reviewed, and the extension thereof by Mittag-Leffler.

The next paper was 'On the Foundations of Geometry and on Possible Systems of Geometry,' by Dr. Henry Freeman Stecker, of Cornell University. In the absence of Dr. Stecker his paper was presented by Mr. Radelfinger.

After an introduction on the assumptions which must be made in constructing a geometry, Dr. Stecker reviewed the criticisms of Moore and Schur of Hilbert's classic paper of 1899, recently translated, and announced the concluson that in spite of all criticisms and attempted improvements, Hilbert's system has 'withstood all attacks, and remains not only apparently sound in logic, but the simplest of such systems as have thus far been constructed.'

An account was next given of Hilbert's second, and recent, great memoir, *Mathematische Annalen*, Bd. 56, which has for its object to establish Lie's well-known and indispensable results, without the assumption, made by Lie, that the functions defining the displacements are differentiable. In solving the problem Hilbert makes use of Cantor's theory of point-assemblages and Jordan's theory of a closed curve free from double points. Hilbert's results, so far as they go, establish the independence of Lie's results of the assumption stated above, but they have yet to be extended to elliptic geometry and also to space.

In conclusion a thesis by Hamel, a pupil of Hilbert's was discussed, which leads to the conclusion that 'from the standpoint of the calculus of variations the Euclidean geometry is the simplest possible.'

Charles K. Wead,
Secretary.

NEW YORK ACADEMY OF SCIENCES.

SECTION OF ANTHROPOLOGY AND PSYCHOLOGY.

The regular meeting of the section was held February 23, in conjunction with the New York branch of the American Psychological Association, Professor Thorndike presiding. Afternoon and evening sessions were held, the members dining together at the close of the afternoon session. The following papers were presented:

'Phonetic Surveys,' Professor E. W. Scripture. After brief mention of the phonetic surveys being carried on by Grierson in India and Guilleron in France, a description was given of the chief talking machine methods that may be used for this purpose. It was pointed out that the advances in the construction of phonographs, graphophones and gramophones during the last couple of years have been so great as to revolutionize these methods. It was also explained that making a speech record was like taking a photograph; everybody can take a picture, but a good picture requires skill. By use of the grapho-

phone the records made on wax cylinders can be used for making metal molds; from these indestructible molds copies in hard wax can be made. The gramophone method likewise furnishes metal molds from which hard discs are produced; but the talking machine requires an expert. This gramophone method was lately used on three expeditions sent out by the Vienna Academy of Sciences. The new methods furnish records that are perfect in recording every detail of the voice. There is not the slightest loss even of the most difficult consonants. Criticisms stating the contrary are derived from acquaintance with methods that are now out of date. From the gramophone records the curve of speech can be traced off with great accuracy; whereby every detail of the voice can be measured. A similar method can be applied to phonograph records. It was urged that the fast disappearing dialects and languages should be recorded and preserved in one of these ways. It was pointed out that such records could be made and delivered at smaller cost per word than in the case of Guilleron's 'Atlas.' It was stated that the various talking machine companies have shown self-sacrificing interest in such work, and that the Victor Talking Machine Company would be willing to loan its record-taking car when it is finished. Exhibits of various material and speech curves were made.

'Correlations of Measurements of Growth,' Dr. Clark Wissler. (Read by title.)

'Correlations in School Children,' Dr. J. H. Bair. The measurements were taken on Worcester school children. A high coefficient of correlation was shown between stature and height-sitting, stature and weight, and height-sitting and weight. Between stature, height-sitting, weight, with length of head and width of head the amount of correlation was much less and much more irregular than between the measurements first named. This irregularity was partly due to the small number of cases examined.

'Apparent Motion in Stereoscopic Vision,' Professor J. E. Lough. Stereoscopic pictures may be united without the aid of a stereoscope, *i. e.*, by direct fixation, whenever the distances between similar objects in the two pictures is not greater than the interocular distance. When pictures are so united—giving a direct perception of the third dimension—any movement of the picture from side to side gives the impression that objects in the background are moving through a greater distance than are the objects in the foreground. This 'slipping' of the background is perceived with still greater vividness when the picture remains stationary and the head is rotated or moved from side to side. In bringing a stereoscopic picture nearer the eyes the background seems to approach more rapidly than the foreground, and in moving the picture away from the eyes the background seems to move away more rapidly. The apparent motion in stereoscopic pictures seen under the above conditions is probably due to the fact that the angle of parallax remains constant, while the line of direction varies, with every movement of the head or of the picture.

'An Experiment in Facial Vision,' Professor Robert MacDougall. The paper supplements and in three respects aims to correct the reports of previous experiments on facial vision. In the perception of objects in proximity to the face independently of the sense of sight, the nature of the sensory impression upon which perception depends is not commonly discriminated. In the present investigation the percentage of correct perceptions was found to lie between 50 and 75, that is, within the subliminal region. This result is contrary to previous work in which the percentage lay clearly above the threshold of 75. If a true perceptual process be involved, the percentage of correct responses should be a function of the absolute differences between the objects discriminated. This was found to be the case in the present set of experiments, but not in preceding investigations. In work published heretofore the perception was reported to be mediated solely by sensations of sound, but in the present investigation the shutting off of auditory stimulation made practically no reduction in the percentage of correct responses.

'Notes on the Washington Meeting,' Professor E. H. Sneath. The Washington meeting, if compared with a possible meeting of psychologists twenty-five years ago, shows the lines along which progress has been made. Such a comparison demonstrates clearly (1) the special training required of the psychologists of to-day; (2) the position of psychology among the sciences; (3) the growth of productive scholarship; (4) the differentiation of the work into experimental, genetic, comparative, abnormal, educational, etc.; (5) the development of new methods of approach.

'Grades for Mental Traits,' Professor J. McKeen Cattell. This paper treated the accuracy with which grades can be assigned for college studies, and the methods to be employed in assigning grades. Those who do well in one study or have one trait in excess are likely to do well in other studies and to have other traits in excess, and they are more likely to succeed in after life. It was shown, however, that the grades assigned to students have not very great validity. It was recommended that grades be assigned in a scale of ten and that a probable error be attached to the grade. The grades should represent groups of equal size rather than equal differences in merit. The paper also discussed the grade assigned to large groups for mental, moral and physical traits, and gave some of the results that the writer had obtained.

'A Preliminary Report on Tests of One Hundred Men of Science,' W. H. Davis. (Read by title.)

James E. Lough,
Secretary.

THE TEXAS ACADEMY OF SCIENCE.

At the regular meeting of the Texas Academy of Science held in the chemical lecture room of the University of Texas on Friday evening, November 28, 1902, the following papers were presented by title:

'Contribution to a Knowledge of the Coleopterous Fauna of the Lower Rio Grande Valley in Texas and Tamaulipas, with Biological Notes and Special Reference to Geographical Distribution,' by C. H. T. Townsend, El Paso.

'Poisonous Snakes of Texas,' by J. D. Mitchell, Victoria.

Mr. E. C. H. Bantel, instructor in engineering, University of Texas, gave an illustrated lecture on 'Iron Smelting.'

At the formal meeting held in the university auditorium on Monday evening, December 29, 1902, Dr. William L. Bray, professor of botany, delivered an illustrated lecture on 'The Evolution of the Flower and its Relations to Insects and other Pollenizing Agents.'

The following papers appeared by title on the program of this meeting:

'The Effect of Weeds and Moss upon the Coefficients of Discharge in Small Irrigating Canals,' by J. C. Nagle, professor of civil engineering in the Agricultural and Mechanical College of Texas, College Station.

'The Decomposition of Potassium Chlorate at Fixed Temperatures,' by Eugene P. Schoch, Ph.D., and J. S. Brown, B.S.

'The Kinetics of Oxidation Reactions. Example I. The Equilibrium between Potassium Ferrocyanide, Potassium Ferricyanide, Iodine and Potassium Iodide,' by Eugene P. Schoch, Ph.D., instructor in chemistry, University of Texas.

'Contribution to the Chemistry of Fatigue,' by Dr. Henry Winston Harper, professor of chemistry in the University of Texas, and Margaret Holliday, M.S.

At the regular meeting held in the zoological lecture room of the university, on Saturday evening, March 14, 1903, the following papers were presented, both of which were illustrated with stereopticon views:

'Some Wholesome Educational Statistics,' by W. S. Sutton, M.A., professor of the science and art of education in the university.

'Steel Making,' by E. C. H. Bantel, C.E., instructor in engineering.

Frederic W. Simonds,
Secretary.

CLEMSON COLLEGE SCIENCE CLUB.

At the regular monthly meeting of the club on Friday evening, February 27, Professor J. V. Lewis presented an illustrated paper on 'The Occurrence and Origin of Corundum in the Eastern United States.'

Corundum occurs in the crystalline rocks of the Appalachian region in granite, gneiss, mica-schist, crystalline limestone, etc., but thus far the only deposits that have been profitably exploited occur in basic magnesian rocks, chiefly peridotites, with smaller bodies of pyroxenites and amphibolites. The sapphire or gem variety, common corundum, and emery have all been found in this region. The second occurs most commonly with the peridotites, and, except in one or two localities, is much the most important. It occurs at intervals from Alabama to Massachusetts, and has been mined in Pennsylvania, North Carolina and Georgia, occurring chiefly in vein-like zones about the borders of the peridotites and sometimes penetrating the mass of these rocks.

North of North Carolina the peridotites are extensively altered into serpentine, steatite, etc. In North Carolina and southward they are chiefly unaltered. The studies of Lewis and Pratt have demonstrated (1) that the peridotites are igneous intrusives, and (2) that the corundum has most probably been formed by segregation from a state of solution in the molten magma. These results have been briefly presented in a report shortly to be published by the North Carolina Geological Survey.

In the discussion of this paper, Dr. P. H. Mell stated that he had had occasion to investigate these peridotites and corundum deposits in the seventies, when they were first beginning to attract attention. He collected material from which, to the best of his knowledge, the first corundum wheel ever made had been manufactured. He traversed the whole region, from Alabama to Massachusetts, and arrived at the conclusion that both the corundum and the peridotites are of igneous origin, which view was almost unanimously rejected by geologists at the time. Therefore, he was very much gratified to find that the results of the latest investigations of these rocks confirmed his own conclusion.

CHAS. E. CHAMBLISS,
Secretary.
A. AND M. COLLEGE OF SOUTH CAROLINA,
CLEMSON COLLEGE, S. C.

VERMONT BOTANICAL CLUB.

THE eighth annual winter meeting of the Vermont Botanical Club was held at the University of Vermont on January 16 and 17. The officers were reelected for the ensuing year as follows:

President—Ezra Brainerd, Middlebury College.
Vice-President—C. G. Pringle, University of Vermont.
Secretary—L. R. Jones, University of Vermont.

It was the sentiment that the summer field meeting, about July 1, be held on Stratton Mountain.

The following program was presented:

CLIFTON D. HOWE: Annual Address—'Some Results of Deforestation in Vermont.'

PRESIDENT EZRA BRAINERD: 'Vermont Violets.'

WILLIAM H. BLANCHARD: 'More Finds in My Section.'

MISS ALICE E. BACON: 'An Experiment with the Fruit of the Red Baneberry.'

MRS. FREDERICK A. RICHARDSON: 'Reversion in a Columbine.'

MISS ELIZABETH BILLINGS: 'A Many Branched Spleenwort.'

WILLIAM STUART: 'Nitrogen Gatherers.'

MRS. H. E. STRAW: 'Ferns of Smugglers and Nebraska Notches.'

H. M. SEELEY: 'My Aster.'

MRS. E. B. DAVENPORT: 'Recollections of Mr. C. C. Frost.'

L. R. JONES: 'The Frost Herbarium.'

JOHN HENLEY BARNHARDT: 'A List of Vermont Local Floras.'

E. A. BURT: 'The Thelephoræ of Vermont.'

MRS. NELLIE F. FLYNN: 'Additions to the Flora of Burlington and Vicinity during 1902.'

G. T. MOORE: 'The Pollution of Water Supplies by Algæ.'

MISS RUTH B. FISHER: 'An Appeal for More Study of the Lower Plants in our Schools.'

PRESIDENT EZRA BRAINERD: 'The Chandler Herbarium.'

W. W. EGGLESTON: 'A Canoe Trip in Northern Maine.'

CARLTON D. HOWE: 'Plant Progression.'

F. A. ROSS: 'Vagaries of *Hepatica*.'

MISS M. EVA BAKER: 'The Message of the Trees.'

L. R. JONES,
Secretary.

NEBRASKA ORNITHOLOGISTS' UNION.

THE fourth annual meeting of the Nebraska Ornithologists' Union was held in Lincoln, Neb., January 24, 1903, on which occasion the following papers were read:

REV. J. M. BATES: President's address—'Birds and Man.'

MRS. C. S. LOBINGIER: 'Educational Value of Bird Study.'

MISS ANNA CALDWELL: 'Devices for Interesting Children in Bird Study.'

REV. J. M. BATES: 'Observations on the Number of Birds to the Square Mile in Custer County.'

MR. WILSON TOUT: 'The Crow in Nebraska.'

MR. MYRON SWENK: 'The Birds of the Niobrara Valley.'

DR. R. H. WOLCOTT: 'Birds of Cherry County, Neb.

DR. R. H. WOLCOTT: 'Remarks on a Record of Nebraska Ornithology.'

Newly elected members raised the total membership of the society to nearly two hundred.

The following officers were elected:

President—F. H. Shoemaker, Omaha.

Vice-President—Miss Anna Caldwell, Lincoln.

Corresponding Secretary—J. C. Crawford, Jr., West Point.

Recording Secretary and Editor (permanent)—Dr. R. H. Wolcott, University of Nebraska.

Treasurer—Mr. August Eiche, Lincoln.

Executive Committee—Dr. G. E. Condra, University of Nebraska; Dr. H. B. Lowry, Lincoln; J. A. Dickinson, Gresham.

The office of Custodian was created as a permanent office and Myron Swenk, of Lincoln, appointed to fill it.

The presentation of a considerable amount of material, including many skins on which records are based, was reported, and it was resolved to secure, if possible, for the collection, all the material in the state upon which the past records of the occurrence of rare birds in Nebraska had been based.

A committee was appointed to complete the formal organization of the Audubon Auxiliary and to put in definite shape terms of affiliation between it and the union.

ROBERT H. WOLCOTT,
Secretary.

DISCUSSION AND CORRESPONDENCE.

BIOMETRY AND BIOMETRIKA.

TO THE EDITOR OF SCIENCE: May I as one of the editors of the above journal make a personal appeal to the American scientific world through your columns? My reasons for doing so are twofold. In America the novel, be it in science, politics or industry, is not *a priori* condemned as the undesirable or the fatuous, which is its customary fate in Europe. Secondly, the list of American subscribers to our journal shows us that American biologists and mathematicians are willing to consider on its own merits what biometry has to say for itself; they are not from the beginning hostile to the new movement. In Europe the older teachers will have nothing to do with the subject. The list of subscribers shows that we depend chiefly upon the younger workers here; and every difficulty is put in the way of their doing biometric work. This is extremely serious, for it means that appointments and fellowships will not follow on research work in biometry, and thus young scientists are and will be discouraged from taking it up. Quite recently a young American biometrician working here was surprised to find the scorn with which the officials at a great national institution treated his measuring work. 'Well, tell us what biometry has proved?' was the question put to him by officials, whose library contained no copy of the journal, and who apparently had never studied its pages. Another young worker proceeding to take up a colonial appointment was warned by one of the doyens of zoology on all grounds to give up this foolish biometric work. A third, going to work in one of the greatest continental zoological laboratories, finds its world-renowned head disgusted because he attempts to solve a problem by statistics which in fact can and can only be solved in that way.

None of our responsible biometricians claim that there is *one* way only of solving *all* biological problems, but solely that there is only one way, the statistico-mathematical method, in which *certain* problems can be answered. We do not, therefore, aim at depriving biologists, pure and simple, of their field of ac-

tivity. We do not call upon them all to return to school and learn mathematics. What we do say is that, in a certain part of that field, their past conclusions have been based on inadequate reasoning, and we place at the disposal of those who are willing or able to use it a new instrument of investigation. For the many who have not a taste for statistics or mathematics, we have endeavored to provide with each number of our journal a brief summary of what our contributors consider their investigations to demonstrate. A brief examination of these summaries will show the unprejudiced that, on the one hand, no attempt has been made to exaggerate the value of the work done, nor, on the other, has biometry failed to achieve something in the first year during which it has possessed an organ of its own. In the case of the mathematician we have even more to offer than to the biologist; we ask him to see that his science has far wider applications than he has hitherto dreamt of; that a door has been opened to him which quite doubles the space to which he has so far had to confine himself. That in future he may revel in vital phenomena as he has hitherto revelled in the physical universe. That perfect correlation, the causal nexus of the physicist, is only a special case of the general theory of correlation which covers organic as well as inorganic relationships. The older mathematicians worked only under the category of causation, the moderns can also work under that of correlation. In the light of our present knowledge the chapters in treatises on scientific method, which center round 'causation' as the fundamental idea, seem as inadequate as the formal logic of the schools is when compared with 'the numerically definite syllogism.' The mathematician who sees causation as merely a special case of correlation passes—to use a not entirely fanciful geometrical analogy—from space of two to space of three dimensions. Anthropology, craniology, psychology and child study and pedagogy, as well as biology, become fields of work crowded with new problems for the mathematician to tackle; nor must the workers in those fields look upon him as an 'undesirable alien.' He comes to enrich their own domestic industries with new processes and show them how to handle new and powerful instruments of research. There are signs—very hopeful signs—that this broader view of cooperative action is beginning to be realized in America; it will take a generation or two to produce much effect upon the conservative minds of European scientists, who, circumscribed by an over narrow specialization in field and method, are only annoyed if one suggests that for certain tasks the steam plough is more effective than the spade. In England this great reluctance to adopt new ideas is as manifest in science as in commerce and national defence; it is leaving our race behind in both education and industry.

My appeal accordingly to the American scientific world would be of a double nature. Our journal must perforce have an uphill struggle for the first few years of its existence; we are determined to see it successfully through that struggle, but this task can be made a good deal easier for us by both material and intellectual sympathy from your side of the Atlantic. There are many growing public and academic libraries in America; we feel convinced that they will need *Biometrika* ten or fifteen years hence. They will make our task lighter if they aid now by subscribing in our infancy. I would make a like appeal to both biologists and mathematicians; we want additional subscribers, and we want to be studied and read, and not condemned *a priori* without examination. In the next place, the Carnegie Trust gives America a splendid opportunity to judiciously foster new phases of scientific work. I would appeal to those who have to manage that trust not to put hindrances in the way of young men or women who may be seeking to do biometric work. Such persons have got to combine two or three usually separated faculties; they must be moderate mathematicians, intelligent biologists and observant field naturalists. Do not subject them to a severe triple test, or demand that they shall be at the summit of the tree in mathematics and in laboratory work and in field observation. The engineer must know some mathematics, some

physics and some geology; but we don't expect him to be a first-class proficient in all three sciences. We judge him finally as an engineer. So it must be in biometry. No one can get on without some mathematics, some biology and some field work in this new science; but its workers must be ultimately judged as *biometricians*, and not as mathematicians or biologists. Don't allow, however great their reputation or authority, the pure mathematician or the descriptive biologist, who may never have done a stroke of biometric work, to override biometric workers' claims to recognition. Remember that we have here a new branch of science, which has its own methods and its own disciples. Like all young things, it has its future before it, and no amount of step-motherly treatment will, in the long run, profit the reputation of the scientific community which practices it. In the matter of biometry, America has not yet adopted a hostile attitude. I write in the hope that it may never do so.

Karl Pearson.

University College, London, England.

THE DESTRUCTION OF FROGS.

Apropos to the note of Mr. Albert M. Reese, relative to the destruction of frogs, I will say that I once witnessed the same thing in Columbus, Ohio, along the Neil Avenue Street Railway. It was in spring, and the frogs had evidently migrated from the Olentangy River, a short distance away and running parallel with the avenue. I did not count them, but there were very many that had been crushed under the car wheels within a distance of perhaps one fourth of a mile. As I recall, the frogs were crushed across the middle. My observations were made in the morning and I inferred that the migration had taken place either in evening or early morning.

H. A. Weber.

A RARE SCIENTIFIC BOOK.

To the Editor of Science: There is a copy of Purkinje's 'Commentatio de examine physiologico,' etc. (concerning which Professor Wilder inquires in the issue of Science for April 3) in the Library of the Surgeon General of the War Department at Washington.

F. W. Hodge.

Washington, D. C.,
April 4, 1903.

THE IMPROVEMENT OF THE MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

To the Editor of Science: The changes in the arrangements for the meetings of the American Association, proposed by Professor Davis in Science, pages 428–430, and of which I heartily approve, lead me to make the following suggestion that can be carried out easily by the secretaries of the sections.

At the entrance to each sectional meeting-place, let a doorkeeper write upon a blackboard the paper then being read or discussed, and also the paper that will be called next. It is usually impossible for a section to follow the daily program, as printed, or even to restrict the papers to the time allotted to each; therefore, the expedient suggested will obviate the embarrassment to the speaker, as well as the distraction of his audience, caused by the frequent entrance and exit of persons who merely desire to ascertain what paper is being read, and, by showing this at a glance, it will aid such people as wish to hear certain papers in several sections meeting simultaneously.

These bulletins of the current communications are commonly employed at the meetings of the British Association for the Advancement of Science, where they are regarded as so useful that there has been general complaint whenever they have been inadvertently omitted. If tried at the St. Louis meeting of the American Association, I am convinced that we also shall adopt this custom permanently.

A. Lawrence Rotch.

Blue Hill Observatory,
March 18, 1903.

SHORTER ARTICLES.

THE OCCURRENCE OF THREE INTERESTING FISHES ON THE NEW JERSEY COAST.

MANTIDÆ.

1. Manta manatia (Schneider).

1792. *Raja, birostris, rostro bifido* Walbaum, Pet. Arted. Gen. Pisc., III., p. 535 (based

on Zee duiivel, Diabolus marinus Willughby, Hist. Pisc., App., 1686, p. 5, plate 9, fig. 3; no locality; pre-linnæan). [Nonbinomial.]

1801. (*Raja.*) *Manatia* Schneider, Syst. Ichth., p. 364 (based on La Raie Manatia, Lacépède, Hist. Nat. Poiss., I., 1798, p. 160; les rivages de l'Amérique voisins de l'equateur; nonbinomial).

1824. *C.(ephalopterus) Vampyrus* Mitchell, Ann. Lyc. Nat. Hist. New York, I., p. 23, plate 2, fig. 1; "near the entrance of Delaware bay, by the crew of a smack. They had heard that creatures of extraordinary form and size, were frequent in the tract situated off Capes May and Henlopen, during the warm season."

1824. *C.(ephaloptera) gigantea* Le Sueur Journ. Acad. Nat. Sci. Phila., IV., p. 115, plate 6, figs. 1–4; taken near the entrance of the Delaware (Coll. Philadelphia Museum).

A large example of this species was taken in the sea, about a mile from shore, at Stone Harbor, N. J., September 1, 1902. It was taken in a pound net with the harpoon by some fishermen, and then dragged ashore behind a whaleboat. When in the net it behaved very quietly until harpooned, after which it created a great disturbance. It soon died, however, when brought on the beach. Nothing was preserved except one of the eyes and a small piece of the skin. The former measures about two and one eighth inches in diameter, and its pupil about eleven sixteenths of an inch. These are contained in the collections of the Academy.

As Walbaum is nonbinomial, the next available name is that proposed by Schneider. The name in current use, *Manta birostris,* will thus be superseded by *Manta manatia.*

SCOMBRIDÆ.

2. THUNNIS THYNNUS (Linnæus).

1758. (*Scomber.*) *Thynnus* Linnæus, Syst. Nat., Ed. X., p. 297; inter Tropicos, in Pelago (based on *Scomber pinnulis,* etc., Artedi, Ichth., 1738, p. 31; no locality, evidently the great tunny of Europe).

I examined a large example, a little over eight feet in length, which was said to have weighed 700 pounds. It was brought to the Philadelphia market November 4, 1898, from near Brighton, N. J., where it was taken in the ocean. No attempt was made to use the flesh, and it remained on exhibition for several days.

CEPHALACANTHIDÆ.

3. CEPHALACANTHUS VOLITANS (Linnæus).

1758. (*Trigla.*) *volitans* Linnæus, Syst. Nat. Ed. X., p. 302; in Mari Mediterraneo, Oceano, Pelago inter tropicos, in Asia, imprimis ad Cap. b. spei, sape agitata evolans ex aqua (part; based in *Trigla capite,* etc., Artedi, Ichth., 1738, p. 44; mare Mediterraneum).

A large example of this species was taken at Holly Beach, October 11, 1902. It is now in the collection of the Academy of Natural Sciences of Philadelphia.

HENRY W. FOWLER.
ACADEMY OF NATURAL SCIENCES,
PHILADELPHIA, PA., January 17, 1903.

CURRENT NOTES ON METEOROLOGY.

BIGELOW'S BAROMETRY.

VOLUME II. of the 'Report of the Chief of the Weather Bureau for 1900–1901' is an elaborate 'Report on the Barometry of the United States, Canada and the West Indies,' prepared by Professor F. H. Bigelow. The volume numbers 1,005 quarto pages, and contains 55 tables and 39 charts. The need of some revision of the barometric observations becomes apparent when it is recalled that these observations have not hitherto been reduced to a homogeneous system by the application of all the necessary reductions. The method of reduction has also varied from time to time. Four methods of reduction have been employed before the one contained in this report. Professor Bigelow has preserved the Ferrel system of reduction, has 'added another for local abnormality, computed the effect of the vapor pressure separately from that of the free air, and discussed thoroughly the temperature argument, so that these, added to the usual free-air reduction, give the ones required for the plateau districts.' Some idea of the scope of the report may be gained

from a brief enumeration of a portion of its contents, *e. g.*, new barometric reduction tables for reductions between any two planes within the elevations from sea level to 10,000 feet; the construction of temperature gradients in latitude, longitude and altitude for all stations of the United States; the computation of the vapor tension on the sea level, the 3,500-foot and the 10,000-foot planes; the construction of charts of pressure, temperature and vapor tension for each month and the year on the three planes just mentioned; the preparation of special station tables for the practical work of reducing the observations to sea level, to the 3,500-foot plane, and the 10,000-foot plane for the daily weather maps; the compilation of tables giving the normal values of the pressures, temperatures and vapor pressures at the stations and on the three planes. The volume is one of the most important publications of the United States Weather Service since its establishment.

METEOROLOGICAL OBSERVATIONS IN BOSNIA.

In the *Meteorologische Zeitschrift* for January, Hann discusses the observations (1895–1901) made at the observatory on Bjelasnica, a mountain 2,067 meters high in Bosnia—interesting because it is situated further to the southeast than any high-level station in Europe. The pressure, as in the case of all mountains in the Temperate Zone, rises considerably from winter to summer. The winters are abnormally cold, even as compared with the mountain stations in somewhat higher latitudes, the explanation undoubtedly being that Bjelasnica lies on the south or southeast of the winter barometric maximum over the Alps, and consequently its winds are north, northeast or east. The frequent inversions of temperature, and the bright dry days, which characterize the Alps in winter, and help so much to produce the remarkable winter climate of many of the higher Alpine stations, are conspicuous by their absence on Bjelasnica. Very remarkable frost formations are observed in winter, rivaling those of Ben Nevis and the Brocken, which have often been described and photographed. On February 20, 1902, for example, the frost needles reached a maximum length of 2.8 meters, after a growth of three days with light southerly winds; a mean temperature of 26°.6, and a mean relative humidity of 93 per cent. Several needles over three meters long broke off partly by their own weight, and partly because of the wind. To the neighborhood of the Adriatic Sea these extraordinary frost formations are due, as, in the case of the Brocken and of Ben Nevis, they are due to the proximity of the Atlantic Ocean.

HIGH WINDS ON THE PACIFIC COAST.

High winds are not commonly associated with Pacific Coast meteorology. Hence an account of some high-wind records on that coast, in the Annual Report of the California *Climate and Crops,* is of interest. These records were made at the new Weather Bureau station at Point Reyes Light (lat. 38° 12′ N., long. 122° 51′ W.). On February 23–25, 1902, during a 'severe southeast disturbance' along the coast of California, velocities up to 100 miles an hour were observed. On March 1 the wind blew for a few minutes at the rate of 107 miles an hour. Between May 15 and 20, 1902, with a marked depression over the Mexican boundary and the valley of the Colorado, the maximum wind velocity was at the rate of 110 miles an hour.

R. DeC. Ward.

SCIENTIFIC POSITIONS UNDER THE GOVERNMENT.

The civil service commission announces a number of examinations for positions in the scientific service of the government. On April 21 and 22, there will be an examination for the position of aid in the Coast and Geodetic Survey, the age limit being from 18 to 25 years, and the salary $750 and traveling expenses. There are twelve vacancies to be filled by this examination.

On May 5 there is an examination for the position of computer for nutrition investigations in the Office of Experiment Stations, at a salary ranging from $720 to $1,000.

On May 5 and 6 there is an examination for the position of field assistant in forestry at a salary of $1,000.

On May 16, there are examinations for positions in the Bureau of Plant Pathology Industry of plant physiologist, at a salary of $1,500; of plant pathologist, at a salary of $1,600; of viticulturist, at a salary of $1,600; and of physiological chemist in cereal investigations, at a salary of $1,500. Further details concerning these and similar examinations can be obtained from the U. S. Civil Service Commission, Washington, D. C.

SCIENTIFIC NOTES AND NEWS.

PROFESSOR HERMANN STRUVE, of Königsberg, has been given the gold medal of the Royal Astronomical Society.

THE University of Halle has conferred a gold medal on Professor J. P. Pawlow, of St. Petersburg, for his research on digestion.

THE Institute of France has awarded to Dr. Emile Roux, the subdirector of the Pasteur Institute, the prize of $20,000, founded by M. Daniel Osiris, for the person that the institute considered the most worthy to be thus rewarded. It is said that Dr. Roux will give the money to the Pasteur Institute.

DR. F. SCHAUDINN, of Berlin, has been awarded the Tiedemann prize by the Senkenburg Society, at Frankfurt a. M., for his biologic studies and monograph on the Coccidia.

M. T. H. SCHLOESING, JR., has been elected a member of the Paris Academy of Sciences in the section of agriculture in the place of the late M. Dehérain.

WE noted last week the election of Dr. Robert Koch, as foreign associate of the Paris Academy of Sciences, in succession to Professor Rudolf Virchow. It may be added that Dr. Robert Koch received twenty-six votes, Dr. Alexander Agassiz eighteen votes, Dr. S. P. Langley six votes and Professor van der Waals, of Amsterdam, one vote.

PROFESSOR SOLMS-LAUBACH, of Strasburg, and Professor K. Goebel, of Munich, have been elected honorary members of the Botanical and Zoological Society of Vienna.

PROFESSOR H. A. SURFACE, of the Pennsylvania State College, has been appointed economic zoologist to the state of Pennsylvania.

DR. LEONARD P. KINNICUTT, professor of chemistry at the Worcester Polytechnic Institute, has been appointed consulting chemist to the Connecticut State Sewage Commission.

MR. E. E. EWELL has resigned the position of assistant chief of the Bureau of Chemistry, U. S. Department of Agriculture, for the purpose of accepting the position of manager of the Atlanta office of the Propaganda Department of the German Kali Works. Mr. Ewell's service in the Department of Agriculture dates from August, 1889.

PROFESSOR J. A. EWING, F.R.S., professor of mechanism and applied mechanics at Cambridge University, has been appointed director of naval education, Great Britain.

MR. T. H. HOLLAND has been appointed director of the Geological Survey of India, in succession to Mr. C. L. Griesbach, who has retired.

PROFESSOR H. H. DONALDSON, of the University of Chicago, has been elected president of the Chicago Neurological Society for 1903.

MR. ALFRED NOBLE has been elected president of the American Society of Civil Engineers.

PROFESSOR JOHN M. COULTER, head of the Department of Botany, in the University of Chicago, will be absent in Europe during the spring, summer and autumn quarters.

DR. J. MARK BALDWIN, professor of psychology at Princeton University, sailed for Europe on April 4.

DR. JOHN MARSHALL, professor of chemistry in the Medical School of the University of Pennsylvania, has been granted leave of absence and will spend the time abroad.

DR. N. L. BRITTON, director-in-chief of the New York Botanical Garden, and Mrs. Britton returned from Cuba on April 4. During the brief visit made to the island a large collection of herbarium specimens was made principally from the region around Matanzas, and some

desirable specimens were brought back for the conservatories. Valuable assistance was rendered by Mr. John Shafer, curator of the herbarium of the Carnegie Museum, Pittsburgh. In addition to the great amount of material secured which will be of great value in the continuation of investigations upon the flora of the West Indies Dr. Britton was so fortunate as to secure a number of rare botanical books not previously in the library of the garden. Mr. Percy Wilson, museum aid, returned from Honduras, March 18, bringing a large collection of living and prepared specimens of plants from the region near Puerto Sierra and Puerto Cortez.

Dr. James B. Overton, professor of biology at Illinois College, Jacksonville, Illinois, on a grant from the Carnegie Institute as research assistant, will spend the next collegiate year in study and investigation at the University of Bonn, at the special request of Professor Eduard Strasburger, the director of the Botanical Institute.

R. M. Strong, Ph.D. (Harvard, 1901), instructor in biology at Haverford College, 1902–3, has been appointed to a Carnegie research assistantship with Professor C. O. Whitman at the University of Chicago.

Mr. A. D. Selby, botanist to the Agricultural Experiment Station at Wooster, Ohio, who has been in residence at the New York Botanical Garden since December, has been granted a research scholarship of the Garden.

Dr. Thomas M. Balliet, who for fifteen years has been the superintendent of public schools in Springfield, Mass., has recently been honored with a banquet by some one hundred and fifty of the leading men of that city. After many appreciative speeches the affair closed with the presentation to Dr. Balliet of a handsome set of the works of the late Dr. John Fiske.

Lieutenant Koltschok has been sent, by the St. Petersburg Academy of Sciences, to search for Baron Toll, who, last June, left the major part of his polar expedition, and with a few companions proceeded to Bennett Land.

A committee has been formed in Paris with M. H. Moissan as chairman to strike a medal in honor of the late M. P. P. Dehérain, formerly professor of plant physiology in the University of Paris. Subscriptions should be sent to M. Pierre Masson, 120 boulevard St. Germain, Paris. Those subscribing 25 francs will receive a copy of the medal.

Dr. Julius Victor Carus, associate professor of comparative zoology at Leipzig, died on March 10, at the age of seventy-nine years.

Dr. Franz Studnicka, professor of mathematics at Prague, died on February 21 at the age of sixty-seven years.

During the past winter geological work has been carried on by the Louisiana State Geological Survey along the following lines: (1) the stratigraphy of the oil- and water-bearing beds has been studied by Professor G. D. Harris and E. F. Lines (C. U., '04); (2) the clay and lignite deposits have been investigated by C. E. Smith, A.M. (C. U., '03); the subject of terrestrial magnetism has been in charge of Edwin Smith, detailed from the Coast and Geodetic Survey. Through cooperation with the U. S. Geological Survey, an area 1,000 miles square is now being mapped topographically about Baton Rouge. Professor Harris has been engaged for the coming summer by the Hydrographic Division of the U. S. Geological Survey to prepare, by office and field work, a monograph on the underground waters of southern Louisiana. Mr. Lines will be employed by the state to continue the collection of data in the field, looking toward the construction of large topographic models of the state for the St. Louis Exposition. Mr. Smith will soon be in the field again on economic work already begun; and later will divide his time between teaching at the State University and survey work. Mr. Joviano Pacheco (C. U.), formerly assistant on the State Survey, is now draftsman for the Southern Pacific Railroad. He will shortly be transferred to the Louisiana Survey force to aid in the construction of models for the St. Louis Exposition.

The Civil Service Commission announces that the examination scheduled for April 21

for the position of scientific assistant has been postponed to April 28.

THE Tenth Congress of Polish Physicians and Men of Science, which was to meet in Lemberg, Austria, July, 1903, has been postponed until July, 1904.

THE French Association of Anatomists is this week holding at Liège its fifth meeting, under the presidency of Professor Swaen.

THE American Institute of Electrical Engineers has arranged to extend certain privileges to those who are regularly pursuing studies in electrical engineering. Under proper recommendations they may be elected as students of the institute. There is a fee of $3 and the students receive the transactions and may attend the meetings.

THE New York Botanical Garden announces a series of lectures to be delivered in the lecture hall of the museum building of the Garden, Bronx Park, on Saturday afternoons, at 4:30 o'clock, as follows:

April 18. 'A Tour of American Deserts,' by Dr. D. T. MacDougal.

April 25. 'The Vegetation of the Florida Keys,' by Dr. M. A. Howe.

May 2. 'The Framework of Plants,' by Dr. H. M. Richards.

May 9. 'Illustrations of Some Features of the West Indian Flora,' by Dr. N. L. Britton.

May 16. 'The Food Supply of Young Plants,' by Professor F. E. Lloyd.

May 30. 'The Color-Variations of Flowers,' C. C. Curtis.

June 6. 'The Streams, Lakes and Flowers of the Upper Delaware, and the Story of the Sundew,' by Mr. Cornelius Van Brunt.

June 13. 'Vegetable Foods,' by Dr. H. H. Rusby.

REUTER'S Agency has received a letter dated from the Scottish Antarctic ship *Scotia*, at the Falkland Islands, on January 22, written by Mr. W. S. Bruce, the leader of the expedition. He says: In a few hours we take our departure for the South. Contrary to my previous intention, I am going to winter the ship if we find a suitable winter harbor, for, on account of the lateness of the season, there will not be time to set up a separate house and set the ship free. We had a most successful passage south, having accomplished the voyage in 59 days, in contrast to 92 days that we took in the Balaena in '92. We could have made a faster journey, but ran at slow speed in order to save coal and also stopped several times for coaling and for testing gear. After some preliminary accidents in handling gear, we are starting with a fairly clear field to commence good observations. We are very fully loaded down, however, with 200 tons of coal and 20 months' provisions, so we shall make as quick a passage across the Cape Horn seas as possible till we get into the smooth ice water. There we are all right and need not fear for the safety of our deck cargo. Systematic hydrometer observations and temperature observations of the surface of the sea from 30 N. have been taken, and those of the River Plate should prove of exceptional interest, since there are most remarkable and rapid changes both in density and temperature associated with strong currents. We have inspected and set up the meteorological station at Cape Pembroke, which should be as good as any in the Southern Hemisphere. This should form a very important sub-Antarctic station. We have sufficient funds to enable us to do this one year's work in the South. Now that we are on a solid basis it would be a great pity to come home before our work is really complete. A second winter, during which the ship could be kept going free, as well as the station, would be most valuable.

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Two further anonymous gifts, respectively $10,000 and $50,000, have been made to Harvard University for Emerson Hall, to be erected for the department of philosophy. The sum of $125,000 has now been subscribed toward the $150,000 required.

THE Missouri state legislature in the session just closed appropriated for the State University at Columbia $75,000 for a physics laboratory and $25,000 for an addition to the chemical laboratory.

ACCORDING to the financial statement of the University of Minnesota for the fiscal year ending July 31, 1902, the 23/100 mill revenue tax of the state levied annually on account of the university is producing about $150,000

per annum, which is the state's annual contribution to the funds of the Land Grant College of Minnesota. The land grant itself provides an income of about $95,000 and the income of the university itself is over $125,000. The state also makes a deficiency appropriation of $35,000. Of the total income of something less than a half million dollars, the state provides $184,000.

A FUND of $10,500 has been subscribed for Harvard University to establish a lectureship in memory of Edwin L. Godkin, Harvard '71, long editor of *The Nation* and the New York *Evening Post,* who died in March, 1902. The lectures are to be on 'The Essentials of Free Government and the Duties of Citizens.'

THE exercises connected with the opening of the new building of the Department of American Archeology of Phillips Academy, Andover, Mass., were held on Saturday, March 28. The address of the day was given by Frederick Ward Putnam, LL.D., of Harvard University, and about five hundred guests interested in the subject and the institution were in attendance. The foundation of a department of archeology in a preparatory school is unusual if not unique, but it is believed by the trustees that the educational value of the courses and the desirability of early training of future workers in the field both warrant the experiment.

THE New Haven correspondent of the New York *Evening Post* writes: The plan of the academic faculty to exclude Greek, Latin, and mathematics from freshman year as required studies contemplates the increase of the present five required freshman studies to eight, of which five must be chosen. Modern languages are divided into French and German electives, and chemistry and history are added. It is ascertained that the corporation at its last meeting sent back the plan—after the faculty had adopted it—with a suggestion which, if accepted, would have prescribed seven freshman studies with choice of five. This modified plan would have compelled a choice of Greek, Latin, or mathematics, but the faculty has refused to accept it. At the next meeting of the corporation the matter will be voted on, and a number of that body are understood to be opposed to a change which they regard as too radical. Among its opponents are also said to be most of the classical professors. The new question is also raised, somewhat acutely, of the jurisdiction of faculty or corporation in the case.

ANNOUNCEMENT has been made of a pretechnical course at Rochester University, graduates from which will be able to enter the junior year at the Massachusetts Institute of Technology or the College of Mechanical and Electrical Engineering at Cornell.

FURTHER steps are being taken toward the establishment of the University of Hamburg.

THERE are two vacant biological fellowships in Princeton University to be conferred by appointment on graduates of not more than five years' standing in approved American colleges. Candidates may send application, with evidence of fitness, to the Registrar, Princeton University, Princeton, N. J.

DR. EDWIN H. HUGHES, a Boston clergyman, has been elected president of De Pauw University.

THE Boston *Transcript* announces the following new appointments at Harvard University: As instructors, H. C. Boynton in metallurgy, W. E. McClintock in highway engineering and M. A. Read in physiography. Assistants appointed for the same year are: P. R. Curtis in ore-dressing and assaying; R. C. Wells in physical chemistry, W. B. Updegraph and D. W. Howes in mechanical drawing, W. M. Gregory in paleontology and A. P. Larrabee in zoology. The following are appointed to Austin teaching fellowships: L. J. Cole, zoology; J. M. Fox in mining and metallurgy, and F. W. Russe in organic chemistry.

PROFESSOR R. E. SMITH, assistant in botany at the Massachusetts Agricultural College and Experiment Station, has accepted the position of pathologist and assistant professor in botany in the University of California.

DR. OSCAR EMMERLING has been promoted to an associate professorship of botany in the University of Berlin.